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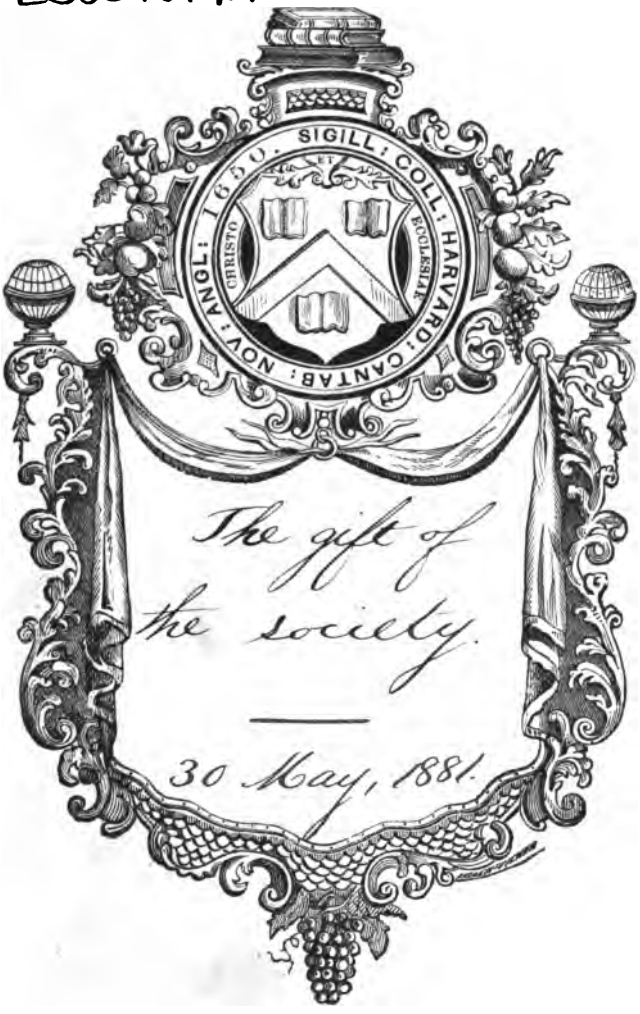
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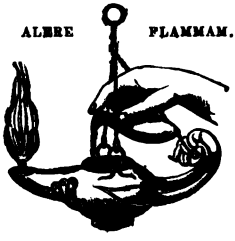

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NOTE.

The Authors of the several Papers contained in this Volume are themselves accountable for all the statements and reasonings which they have offered. In these particulars the Society must not be considered as in any way responsible.

MEMOIRS

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY

OF MANCHESTER.

I. *A Descent into Elden Hole, Derbyshire.*

By Rooke Pennington, LL.B.

Read January 26th, 1875.

Near the road from Buxton to Castleton, and about four miles from the latter place, stands Elden Hill. It is a bleak, bare mountain, one of the highest of the grassy eminences of the Derbyshire limestone district, and, though uninviting itself, commands a most extensive and fine view. Its summit was, long ago in the prehistoric past, chosen as a resting-place for the bones of some savage chief; and in its side is Elden Hole.

Elden Hole is a perpendicular chasm in the rock, and, like many such apertures, is reputed to be bottomless: and, indeed, the sound of a stone falling into the gulf is almost enough to justify the reputation; for, bounding from side to side and smashing as it bounds, the noise of each leap gradually diminishes, but the stone is never heard to stop. Though still famous, it is not so famous as it once was. It was once reckoned one of the seven wonders of the Peak.

In the time of Queen Elizabeth the Earl of Leicester is said to have let a man down into it, who was drawn out speechless, and who shortly afterwards died. A cat, subsequently lowered a considerable distance, was also brought out dead. A hundred years later, Cotton, the poet of the Peak, tried unsuccessfully to fathom it; he has recorded his experiment in verse. At length, just a hundred years ago, Mr. Lloyd, F.R.S., went to the bottom himself and returned safely. His account of what he saw is printed in the 'Philosophical Transactions.' But Mr. Lloyd was a man before his time, and the country-side still went on believing in Leicester's speechless man and dead cat rather than in the scientific explorer; and Elden Hole remains an unfathomable abyss to this day in the belief of the peasant and the tourist; and its glory has departed, not through diminished depth, but because it is near no railway and even adjoins no carriage-road, and the nineteenth-century traveller rarely visits beauties or wonders, the way to which can only be travelled on foot.

On the 11th of September 1873 we explored the chasm for ourselves.

A number of stout beams and planks had been brought up the day before; and of these a rude platform was constructed. We found it was impossible to make this platform and place our windlass so as to obtain a descent plumb to the bottom, or rather to the first landing-place in the chasm, inasmuch as the northern end is the only part where such a drop can be obtained, and there the gulf was much too wide to be bridged over. Having made all our arrangements, we commenced our descent. My friend, Mr. J. Tym, of Castleton, was the first to go down. He was let down for about 15 or 20 yards before coming into contact with the projecting side of the gulf. For about another 10 or 12 yards he slipped over the rock, which, however, was

perfectly smooth, so that there was no risk of cutting the rope. He sustained no further injury than that which befell those of us who followed him, viz. a complete rolling in mud derived from the damp and slippery rocks. As the pioneer, however, he ran considerable danger from stones which had lodged on ledges of rock and which there was a risk of disturbing. When a little more than halfway down, he came clear of the rock again, and there was a sheer descent to the bottom, the rope continuing to run over the smooth projecting side. Three of us followed him, one at a time, each of us being tied to the rope so as to have the hands free to guide the body.

The effect of being lowered into the dark abyss, with the blue sky above and the green ferns and creepers around, was very fine; but the knocking one got against the rock a few yards down soon distracted the attention from scenic effect. At a distance of 180 feet from the top a landing-place was reached, although not a very secure one, as it was inclined at an angle of about 45 degrees. Thence a cavern ran downwards towards the south or south-east; the floor was entirely covered with loose fragments of limestone, probably extending to a considerable thickness.

This is, no doubt, to some extent natural, but is principally artificial, being the result of the favourite amusement of visitors, the throwing down of loose stones from the projecting wall which surrounds the top. The farmer told me that during his time two or three walls had disappeared and been replaced; no doubt it had been the same in the times of his predecessors. There was quite sufficient light at this point to enable one to sketch or read. Having refreshed ourselves, we left daylight behind, and scrambled or, rather, slipped into the cavern. For some few yards, during which we descended a considerable distance, it was of a tunnel-like shape; then it suddenly expanded into a magnificent

hall about 100 feet across and about 70 feet high. The floor of this hall sloped like the tunnel, and, like it, was covered with débris. At the lower side we were about 60 feet below our landing-place, and therefore about 240 feet beneath the surface. The entire roof and walls of this cavern were covered with splendid stalagmitic deposits. From the roof were hung fine stalactites, whilst the sides were covered with almost every conceivable form of deposited carbonate of lime. In some places it was smooth and white as marble, in other places like frosted silver, whilst the rougher portions of the rock were clothed with all sorts of fantastic shapes glistening with moisture. When we had lighted some Bengal fire, I need hardly say the effect was exquisite. From this cavern we could find no opening of any length or depth save the one by which we had entered it, although we very carefully explored it. There is an absurd local tradition of an old woman's goose which flew down, and was given up as lost, but which subsequently reappeared at the mouth of the Peak Cavern, at Castleton. The sagacious and enterprising bird must have been much cleverer than we were.

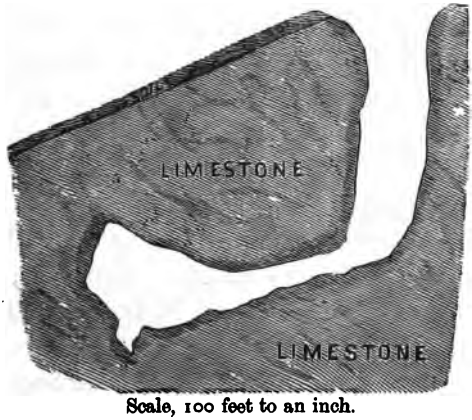
There can be no doubt that this chasm has been formed by the chemical action of carbonic acid in water, and that it has attacked this particular spot either from the unusual softness of the rock originally situated here, or because there was here a joint or shrinkage in the strata. There is nothing, however, in the position of Elden Hole to lead one to suppose that any stream has ever flowed through it; no sign of such a state of things appears anywhere around.

In this it differs from the numerous water-swallows of the neighbourhood, and from the pot-holes of North-West Yorkshire. It is not related to any valley or ravine, or to any running water; and there is, as observed, an absence of any well-defined exit for water at the bottom. No

mechanical action of a flowing stream, therefore, can have assisted the process of enlargement.

That so deep a chasm should be entirely isolated is certainly remarkable, because it cannot be said to represent a "weak point towards which the rainfall has converged." It must therefore be due to the gradual silent solvent properties of rain-water falling on the surface and escaping through jointings and insignificant channels in the hard rocks below. Whether the excavation took place from above or below is uncertain. Applying the rule "a ravine is a cavern open to the sky," such an abyss is simply a perpendicular cave whose roof has at length fallen in. But there are many shallow funnel-shaped depressions in the limestone to which this rule will not apply, and which have been begun from the top; and there are also holes of very

Section of Elden Hole, Derbyshire, September 1873.



Scale, 100 feet to an inch.

irregular shape, apparently of downward origin. So far as the perpendicular shaft of Elden Hole is concerned, it is not at all unlikely that the excavation descended from the surface; on the other hand, there are many underground

chasms which have probably been excavated upwards, and which, when uncovered, will present very much the appearance of Elden Hole. For example, the upper part of the great chasm in the Speedwell Mine bears a strong resemblance to it, though there the process is not yet complete; and could sections of the district south of Castleton be made, those hollow mountains would probably reveal many such a cavity.

II. *Certain Lines observed in Snow Crystals.*

By Arthur W. Waters, F.G.S.

Read January 26th, 1875.

The crystalline form of water belongs to the hexagonal system; and the best condition, but not the only one, for observing this is presented by snow crystals, or, perhaps more correctly, ice crystals; for with a very low temperature there are frequently beautiful ice crystals floating in the air when there is no snow-fall.

Of such some snow is formed, but not all. Dr. Nettis, of Middleburg, published some drawings in 1740; Scoresby in 1822* figured about one hundred forms from the Arctic regions; and Glaisher, in 1844, gave some very elaborate drawings in the Journal of the Microscopical Society. The figures from both these two last sources have been very largely copied for various text-books and scientific works, and are probably familiar to most present.

These figures are made up of angular lines, which seems

* Account of the Arctic Regions.

quite natural for crystalline forms. Some such ice crystals which I observed and figured from Davos in Switzerland*, show a variety of curved lines, the cause of which I found for some time enigmatical; but the explanation is so extremely simple that I feel surprise that it did not at once present itself to me, and, I believe, shows that such observations may have a scientific utility.

It is not often in England that there are favourable opportunities for observation, as the temperature is seldom much below freezing. Frequently in the Swiss mountainous districts, when a very low temperature obtains, there are a great number of the ice-crystals just mentioned floating in the air. These are usually the most beautiful and regular, and are the most readily examined.

The illumination I used was Wenham's parabolic reflector. The lines which I wish to bring before your notice are the curved internal ones, especially such as the meander line in fig. G.

Fig. G.

This crystal I carefully watched during the process of melting. First the external arms melted down as far as this meander line; next the crystal melted down to

* Klimatologische Notizen ü. d. Winter im Hochgebirge von Arthur W. Waters. Basel, 1871.

the straight-sided hexagon; this then melted further, leaving a crystal with the shape of the next interior lines. When we see it taken to pieces we may judge that it was built up in somewhat the same manner—that is to say, by a growth from one form to another; so that, on the simple tabular hexagon, growth takes place along the six sides, or from the corners arms may be thrown out; and such changes may, as my figures show, be repeated several times. But, to explain the curved line found in the interior, we must examine a little further. When a crystal begins to melt, it loses the angular corners and gradually melts down, so that we can suppose a crystal G, thawing down, to take the shape of the meander line. This brings us to the explanation of these curved lines, which indicate, in the first place, a thawing; but as the arms extend beyond these lines, it is clear that after this partial thawing, freezing has begun again.

The production of these lines thus shows that the crystals, after having been formed in a cold atmosphere, have passed into a warmer one, where part of the crystal has been removed by thawing; after this it has again passed into colder air, and has formed anew upon the partially melted crystal.

It seems to me that these crystals bring down with them registers of the comparative temperature of regions inaccessible to the meteorologist, and that a systematic examination in favourable localities would add much to the knowledge of this science, as we may thus learn where countercurrents exist. To take the crystal G, which fell on the 22nd of December 1870, the meteorological observations for Switzerland show that the prevailing wind east of Davos was north or north-east, with thermometer falling, while to the west the wind was south and west. The conclusion which I have arrived at from the lines under con-

sideration is, that above Davos a layer of this southerly wind intruded into the colder northerly winds; and the observations of other days do not militate against this theory; but as I have been able to compare but few days, I should be glad to see a series of observations undertaken to obtain further information.

In Switzerland, the wind-observations are taken by the anemometer placed near the meteorological station. Now many of these are in the valleys; and as the valley-winds are so frequently merely local, these observations have little value for our purpose, and the same for general meteorological purposes. On January 9th, when some of the crystals were observed, the wind registered in the valley at the meteorological station was north, while the clouds (which I always noticed for my observations) showed that a south wind was blowing above.

I have called attention to the fact that I did not find the classification of Scoresby* always correspond with what I observed, finding sometimes low-temperature forms on the coldest days. This may probably be explained by the fact that in the Arctic regions there are fewer counter-currents, and so the crystals were formed under more uniform circumstances: yet at the same time the only explanation of the variety exhibited in each crystal must arise from growth taking place under slightly altered circumstances; else why should it suddenly change from one type to another?

The amount of wind seems to exert quite as much influence upon the form of the crystal as does the temperature. When there is little motion of the air the crystal can go on building itself up regularly in all six directions; but with a considerable amount of wind the conditions for this

* Voyage to Greenland, 1823.

building-up are more favourable on one side than another; so that we may get showers of acicular snow when a strong wind prevails. On the same principle the one side of a rope may be covered with hoar frost turning in the direction of the wind; but the same thing can sometimes be still better seen on a fir all the leaves of which are covered with acicular hoar frost, these needles in the same way all having a direction against the wind.

The temperature when the crystal figured fell was between $-10°{\cdot}5$ C. and $-11°{\cdot}4$ C.

III. *Notes bearing on Mathematical History.* By Sir James Cockle, F.R.S., Corresponding Member of the Society.

Read October 19th, 1875.

1. To the list of Professor Boole's writings which follows the preface to the Supplementary Volume of his 'Differential Equations' [2nd (posthumous) ed., 1865, pp. viii–xi] may be added a paper, entitled "Mr. Boole's theory of the mathematical basis of logic," published in the 'Mechanics' Magazine' (1848, vol. xlix. pp. 254–255).

2. Mr. Blissard in his "Theory of Generic Equations" (Q. J. of Math. vol. iv. p. 279, vol. v. pp. 58 and 185, see also pp. 184 and 325) has applied "representative notation" very extensively. Professor J. R. Young had noticed (Mech. Mag. 1847, vol. xlvii. p. 627) that the law of Bernoulli's numbers is expressed by

$$(1+\mathrm{B})^n-\mathrm{B}_n=0,$$

if we write the exponents of B below instead of above that symbol. Judging from what purports to be an examination

paper, I see that Prof. Young, examining for the Andrews Scholarship, 1851, at University College, London, again noticed (in question 7) the law.

3. In 'Notes and Queries' (1854, vol. x. p. 48, see also p. 191; and 2nd Ser. vol. viii. p. 465, vol. ix. p. 340, vol. x. p. 162; and 4th Ser. vol. ii. p. 316) I long ago pointed out that Barocius, in the margin of p. 264 of his 'Proclus' (1560), cites the 'Geometricæ Enarrationes' of Geminus, and I suggested that there might yet be some hope of recovering that work. In a comment (*ib.* 2nd Ser. vol. ix. p. 449) on something I had said, De Morgan remarked that he took "enarrationem" as a printer's mistake for "effectionem," if Heilbronner were right, adding that, if there be, as both Petavius and Heilbronner seem to state, a printed catalogue of the manuscripts of Barocius, it would be desirable to revive the knowledge of it. De Morgan states that Petavius is the authority for manuscripts of Barocius being brought to England, and that it may be that a manuscript book of Geminus, which Petavius describes as "nondum editus," yet exists in some English library. One of two strange things must have happened (see De Morgan, *ib.* p. 450): viz. either the minute Petavius omitted the title of the work, if it were given; or Heilbronner preserved a title from some other source, if it were not. I am not aware that the manuscript has been discovered.

4. Lagrange, at page 2 of vol. i. of his 'Mécanique Analitique' (1811), observes that the laws of statics are founded on general principles which may be reduced to three, that of the lever, that of the composition of forces, and that of virtual velocities. He says (*ib.* p. 18, No. 14) that the principle of the lever is the only one which has the advantage of being founded on the nature of equilibrium considered in itself, and as a state independent of motion.

5. Lagrange (*ib.* p. 20) says that it does not appear that the ancients knew of the law of virtual velocities. From what is said by Whewell in his 'History of the Inductive Sciences' (1837, vol. ii. pp. 39 and 42, vol. i. pp. 69, 81, 82, and 94) it seems that the law, so far as it relates to the lever, was known to Aristotle. Whewell considered the physical philosophy of Aristotle (*ib.* i. 25) and of the Greeks (*ib.* p. 33) an utter failure. He apparently regarded the doctrine of the lever (see vol. ii. p. 59, and vol. i. chap. i. sect. 1, p. 91) with greater favour than that of virtual velocities (see vol. ii. chap. ii. sect. 4, p. 39), in connexion with which he makes no special mention of Lagrange (see vol. ii. p. 120). Professor Cayley ('Messenger,' N. S., vol. iii. p. 1) has given a dissertation "On the General Equation of Virtual Velocities."

6. There are two modes of approaching the parallelogram of forces. First, we may, from the composition of motions, through the second law of motion, pass to the composition of forces. Thomson and Tait, fortified by the example of Newton (see their 'Treatise' &c., vol. i. pp. 181, 182, § 255–257), have followed this mode, which they believe to contain the most philosophical foundation for statics. Lagrange, whose writings are a mine of historical information, says (Méc. Anal. i. 13) that the ancients knew the composition of motions, as we see by some passages of Aristotle in his 'Mechanical Questions,' and that the geometers especially have employed it for the description of curves, as Archimedes for the spiral, Nicomedes for the conchoid, &c.

7. Secondly, we may seek to arrive at the composition of pressures, independently of the second law of motion, by processes which are valid whether that law be a law of nature or not, and which would be valid even if we had not any conception of motion, and which, indeed, do not render

it necessary to consider whether pressure does or does not tend to produce motion. Lagrange (Méc. An. i. 19) thinks that the principle of the composition of forces, in being separated from that of the composition of motions, loses its principal advantages; and he, just before saying this, throws out a doubt as to whether a principle used by Daniel Bernoulli in his demonstration was altogether independent of the conception of motion. The whole subject of composition is discussed by De Morgan in his paper "On the General Principles of which the Composition or Aggregation of Forces is a consequence" (Camb. Trans. vol. x. part ii. 1859).

8. Lagrange (Méc. An. i. p. 14, No. 11) observes that, although the principles of the lever and of composition lead always to the same results, it is remarkable that the simplest case for the one becomes the most complicated for the other. He adds (*ib.* No. 12) that we can establish an immediate connexion between these two principles by a theorem of Varignon. Newton's view is noticed by Lagrange (*ib.* No. 10).

9. Thomson and Tait have a special object (Treatise, vol. i. Preface, p. v, p. 141. par. (f), p. 341. § 453), in reference to which Newton's proof may be the more appropriate or elegant. I here use the word "elegant" in the sense in which Austin, in his 'Lectures on Jurisprudence' (vol. ii. p. 365), says it is used by the Roman lawyers. But there is much that is interesting in the other investigations on the subject.

10. Lagrange (*ib.* p. 29) uses the word "moment" in the sense which Galileo (*ib.* p. 20) gave to it, viz. the product of the force into its virtual velocity, noticing, however, another (*ib.* p. 7) meaning, viz. the product of the force into the arm of the lever by which it acts.

11. Whewell (Hist. Ind. Sci. i. p. 93), speaking of a

"matter of obvious and universal experience," says:—"This general fact is obvious, when we possess in our minds the ideas which are requisite to apprehend it clearly. When we are so prepared, the truth appears to be manifest independent of experience, and is seen to be a rule to which experience must conform."

He seems (*ib.* ii. 25) to reiterate this opinion, which, if "appears to be manifest" be taken to mean "is self-evident," will hardly pass unquestioned.

12. Lagrange (Méc. An. i. 4, 5) tells us, in words which I translate as follows, to "imagine a triangular plane loaded with two equal weights at the two ends of its base, and with a double weight at its vertex. This plane will evidently be in equilibrium when supported by a straight line or fixed axis which passes through the middle of the two sides of the triangle; for we may regard each of these sides as a lever loaded at its two ends with two equal weights, and which has its fulcrum on the axis which passes through its middle. Now we may contemplate this equilibrium in another manner, by regarding the base itself of the triangle as a lever of which the ends are loaded with two equal weights—and by imagining that there is a transverse lever which joins the vertex of the triangle and the middle of its base in the form of a T, and of which one end is loaded with the double weight placed at the vertex, and the other serves as the fulcrum of the lever which forms the base. It is evident that this last lever will be in equilibrium on the transverse lever which sustains it at its middle, and that the transverse lever will consequently be in equilibrium on the axis on which the plane is already in equilibrium. But since the axis passes through the middle of the two sides of the triangle, it will necessarily pass also through the middle of the straight line

drawn from the vertex of the triangle to the middle of its base; hence the transverse lever will have its fulcrum at the middle point, and must, consequently, be equally loaded at its two ends. Hence the load supported by the fulcrum of the lever which forms the base of the triangle, and which is loaded at its two ends with equal weights, will be equal to the double weight at the vertex, and, consequently, equal to the sum of the two weights."

13. Whewell, in his 'Mechanical Euclid' (2nd ed. p. 170), says that it will be found that Lagrange's proof, if distinctly stated, involves some such axiom as this:—that "If two forces, acting at the extremities of a straight line, and a single force acting at an intermediate point of the straight line, produce the same effect to turn a body about another line, the two forces produce at the intermediate point an effect equal to the single force." He adds that though this axiom may be self-evident, it will hardly be considered more simple than the proposition to be proved. Without discussing Whewell's criticism, I observe that Lagrange (Méc. An. i. 16, 19) regards forces as quantities which can be added and subtracted, and which may be regarded (*ib.* p. 18) as weights. De Morgan (*loc. cit.*) seems to be of opinion that the proposition that the weight of the whole is equal to the sum of the weights of all the parts is known only by experience.

"Oakwal," near Brisbane,
Queensland, Australia,
July 22, 1875.

IV. *On an Instrument for measuring the Direct Heat of the Sun.* By Professor Balfour Stewart, LL.D., F.R.S.

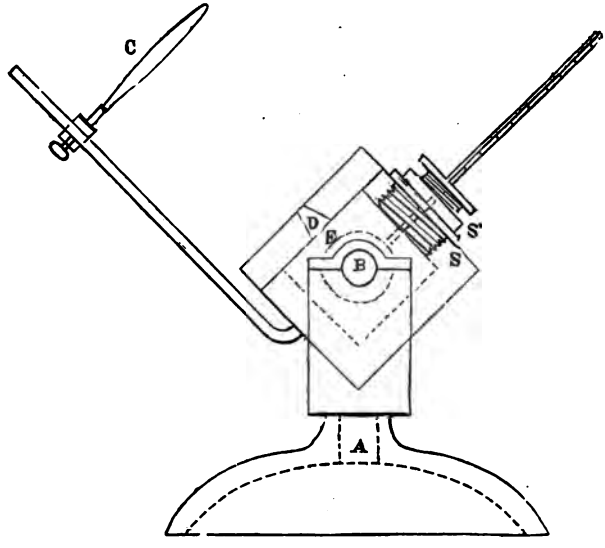
Read November 16th, 1875.

The instrument generally employed for giving the radiant energy of the sun's rays acts upon the following principle. *In the first place,* the instrument is sheltered from the sun, but exposed to the clear sky, say for five minutes. Let the heat so lost be termed r. *Secondly,* the instrument is turned to the sun for five minutes. Let the heat so gained be termed R. *Thirdly,* the instrument, being now hotter than it was in the first operation, is turned once more so as to be exposed to the clear sky for five minutes while it is shielded from the sun. Let the heat so lost be termed r'.

It thus appears that r denotes the heat lost by convection and radiation united when the instrument, before being heated by the sun, is exposed for five minutes to the clear sky, while r' denotes the heat lost by these same two operations by a similar exposure after the instrument has been heated by the sun; and it is assumed that the heat lost from these two causes during the time when the instrument is being heated by the sun will be a mean between r and r', and hence that the whole effect of the sun's rays will be in reality $\mathrm{R}+\frac{r+r'}{2}$.

Now, although this assumption may in the average of a great number of experiments represent the truth, yet in many individual cases it may be far from being true. It would therefore seem to be desirable to get rid of this uncertainty by constructing an instrument in which we are sure that the causes of variability are not allowed to operate.

These causes of variability I have attempted to get rid of in the following manner. With the help of Mr. Jordan, mechanician at Owen's College, the following instrument has been constructed. It consists of a large mercurial thermometer with its bulb in the middle of a cubical cast-iron chamber, this chamber being of such massive material that its temperature will remain sensibly constant for some



time. The chamber, with its thermometer, has a motion in azimuth round a vertical axis A, and also a motion in altitude round a horizontal axis B. A three-inch lens C of 12 inches focal length is attached by means of a rod to the cubical chamber so as to move with it. The nature of this attachment will be seen in the figure. Thus the whole instrument may be easily moved into such a position that the lens as well as the upper side of the chamber (which is parallel to the plane of the lens) may face the sun, and an image of the sun be thrown through a hole D in the side of the chamber upon the thermometer-bulb E.

The stem of the thermometer protrudes from the chamber as in the figure. A screw S, somewhat larger in diameter than the bulb of the thermometer, is made use of to attach the thermometer to its enclosure; and a smaller screw, S′, pressing home upon india-rubber washers, enables the thermometer to be properly adjusted, and kept tight when in adjustment.

In the present instrument the internal diameter of the chamber is 2 inches, while the bulb of the thermometer is about $1\frac{1}{4}$ inch in diameter.

The scale of the thermometer is very open, more than an inch going to one degree. I have generally allowed the image of the sun given by the lens to heat the thermometer-bulb for one minute, during which time an increase of temperature not exceeding in any case two degrees has been produced.

As far as principle is concerned there appears to be no objection to the present instrument; nevertheless it is open to a very serious practical objection. The scale being so very open, the stem comprehends only a few degrees; frequently, therefore, the temperature is such that the extremity of the mercurial column is either below or above the stem. Now the thermometer has a small upper chamber; and by means of a method of manipulation well known to those who work with thermometers, it is possible to add to or take away from the main body of the mercury in the bulb, so as to keep the end of the mercurial column always in the stem. But experience has convinced me that for a thermometer with such a large bulb, frequent manipulation of this kind is not unattended with danger to the bulb. On this account the instrument in its present form is, I conceive, unsuited for steady work in an observatory from year to year.

It is, however, possible, without any appreciable sacrifice

of the scientific principle of the instrument, to alter it in such a manner as to remedy this defect. Without altering the size of the bulb, I should propose for a permanent instrument a stem, say, 18 inches long, with a bore of such diameter that the stem shall embrace a range of temperature between 20° Fahr. and 92° Fahr. Thus somewhat less than five degrees will go to the inch. The stem might be protected from the risk of accident by an appropriate shield. Let such a thermometer be heated for two minutes and the size of the lens be somewhat increased. In this case a rise of something like 5° Fahr. will be obtained; and this heating effect might be very easily estimated to one hundreth of the whole, while the same thermometer will serve for all the temperatures likely to occur in these islands during the course of the year.

I ought to add that a pasteboard cover, gilded on the outside, is made to surround the chamber, and also that between the lens and the chamber there is a pasteboard shield with a hole in it to permit the full rays from the lens to pass—the object of this shield being to prevent rays from the sun or sky from reaching the instrument.

In such an instrument r, or the change taking place in the thermometer before exposure to the sun, will in all probability completely disappear, while r' will be extremely small. At any rate we may be quite certain that $R + \frac{r + r'}{2}$ will accurately represent the heating effect of the sun.

We may probably suppose that in the same instrument the lens (which must always be kept clean) will always stop the same or nearly the same proportion of the solar rays. But the lens of one instrument may not stop the same proportion as that of another instrument. This, however, is no objection if it be borne in mind that the instrument is a differential one. In practice there would be some stand-

ard instrument, which would be retained at a central observatory; all other instruments would, before being used, be compared with it. It would thus be possible to compare together the indications of various instruments working in different places, provided that these, before being issued, had their coefficients determined at the central observatory.

V. *On a Graphical Method of Drawing Spectra.* By Mr. William Dodgson, Whitworth Scholar. Communicated by Professor Roscoe, F.R.S. &c.

Read March 21st, 1876.

Construction of Curve for ascertaining the Wave-lengths.

A number of points are first plotted on the curve-paper, the abscissæ of these points being the micrometer-scale-readings of certain lines as observed directly in the spectroscope, or being absolute or relative measurements taken from an existing photograph; whilst the ordinates of these points are the corresponding wave-lengths, taken from the determinations of some reliable observer.

If there be a large number of points taken, the curve ought not to be drawn actually through them all, or it would probably be irregular; it therefore becomes necessary to draw a mean curve, *i. e.* a continuous even curve which shall pass near to all the given points, some being on one side and some on the other.

The following method was employed to determine the wave-lengths of the well-defined edges of the bands in two absorption-spectra.

The micrometer-scale-readings of 27 air-lines were observed in the spectroscope, in addition to those of the absorbing vapour, and taken as abscissæ, and the corresponding wave-lengths (determined by Thalén) as ordinates. These 27 points, when plotted on the curve-paper, fell naturally into six groups; a mean point was then found for each group by accurate measurement, and the curve drawn through these six mean points, such that the sum of the shortest distances of the several points of any one group from the curve is zero.

The mean points were obtained by means of the arrangement shown in fig. 1.

Fig. 1.

The wooden rod A A has a steel point C C fixed at one end, and a graduated scale of tenths of an inch at the other; the axis of the steel point is kept vertical and the scale horizontal while the rod is being rotated, by the strip of wood T T P, the end P sliding on the table. The centre C being placed at some fixed point, the curve-paper is then adjusted on the table so that some particular division on the scale (found by trial) describes a circle through a group of points, and also through two adjacent groups, one on each side, which circle, if drawn, would pass not far from the mean point of the three groups we are proceeding to determine. The distance from each point of the middle group to the centre C is then read off on the scale to $\frac{1}{1000}$ of an inch, with the aid of a lens and a second scale divided into hundredths of an inch; and a point plotted near the centre of gravity of the middle group at a distance from C equal to the mean of these readings.

This process is repeated for each of the four middle groups, the mean points of an end group being determined with the paper and centre C in the same positions as for the second group from that end.

A nearer approximation to the true position of the mean points may then be found by making use of those already obtained, to find more correctly the radius of the circle through the three groups, and repeating the process of measuring and plotting.

In one case an isolated point occurs; this is taken as one group, and the curve drawn through it.

The curve consists of arcs of circles which pass through the six mean points, with radii varying from $64\frac{1}{4}$ inches at the upper end, to $61\frac{1}{2}$ inches at the lower end, drawn by means of the rod A A, having a drawing-pin fixed to it.

The wave-lengths corresponding to the scale-readings of the observed lines in the two spectra were then found by measurement from the curve to $\frac{1}{1000}$ of an inch, by means of a lens and a scale of hundredths of an inch.

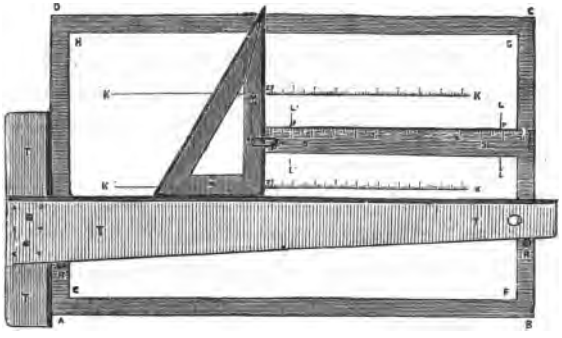
The above method of graphically drawing a curve may be extended to the case where the points are nearly uniformly distributed, and not divided into groups naturally as in the case of air-lines.

Divide the points into any number of groups, about an equal number in each group. Find the mean points as described, by aid of lens and vernier scale on the rod; draw the curve through the points by the best practical method. The number of groups taken will be guided by judgment, and will depend on what the degree of the proposed curve shall be. The greater the number of groups, the greater will be the degree of the equation to a curve passing through their mean points, and the greater probably will be the irregularities in the variation of the radius of curvature.

Construction of Scale for Map of Spectrum.

The arrangement shown in fig. 2 has been employed to draw the map-scale, and found to be an accurate and expeditious method.

Fig. 2.



A B C D, the drawing-board; E F G H, the drawing-paper; T T, Stanley's ebony-edged T-square, resting against the pins R R during the operation; M M, Stanley's ebony-edged set square, which slides along the edge of T T; S S, Stanley's engine-divided millimetre-scale, connected at one end to the edge of the set square M, by pins and elastic string N N, so as to slide with the set square, and have its graduated edge parallel to the edge of T T, and also parallel to the lines K K, K′K′, on which the scales have to be constructed; L L, a very fine ink-line at right angles to the edge of the scale, beyond the extremity of the required scale K K, on the right-hand side, to which the divisions of the millimetre-scale are adjusted by the eye, with the aid of a lens placed on a stand, its axis in a vertical plane through the line L L. A fine adjustment of the line and division is obtained by means of the blunt handle of the drawing-pin, used as a lever against the end of the scale, the surface of the board or the paper being the fulcrum.

The divisions of the map-scale are drawn at Z Z, with a very thin drawing-pin, very close to the edge of the set square; so that the variation of the inclination of the drawing pin is a very inappreciable error. If the map-scale is required to be longer than the scale S, another very fine perpendicular ink line L′ L′ must be drawn at an exact multiple of 10 millimetres distance.

To prevent mistakes in drawing the principal divisions, the line L L is arranged so that they may be drawn when the corresponding principal divisions on the scale S S are coincident with the line.

The advantages of this method of drawing a map-scale arise from the fact that the error due to the observation of the coincidence of two lines is much less than that of plotting a point opposite each division of the millimetre-scale—also in the fact that the error due to drawing a line with a thin pen very close to the edge of the set square (the pen approximately at the same inclination) is much less than that of drawing the divisions through the plotted points, judging by the eye.

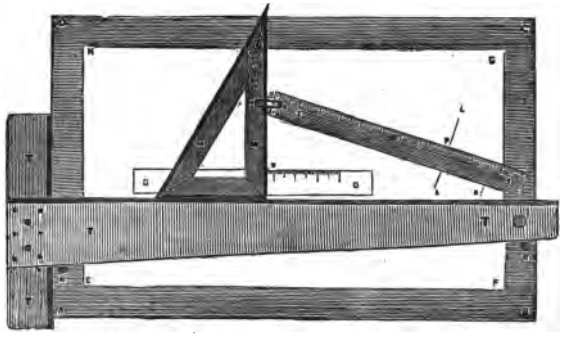
If only one scale be required on the map, place the edge of the scale S near to the line K K. If two are required, place the edge of S S halfway between K K, K′K′, to lessen the errors due to imperfections in the straight edge of the T-square.

Construction of a Vernier Scale for the above Map-Scale.

The drawing-board, T-square, set square, and paper as in fig. 2. O O (fig. 3), a strip of paper on which the vernier scale is to be constructed, its edge being parallel to T T; the scale S S is attached to the edge of the set square, and slides with it, by means of the pins and elastic N N as before, but inclined to the edge of T T at an angle $\alpha = \sec^{-1} \frac{21}{20}$; it is kept

in this position by the pegs U, U in the end of the scale; the fine ink-line L L is drawn at right angles to S S, the

Fig. 3.



lens being used on a stand as before, with its axis always in a vertical plane through L L.

The divisions on the vernier scale will be $1\frac{1}{20}$ millimetre in length.

VI. *The* Eucalyptus *near Rome.*

By Dr. R. Angus Smith, F.R.S., V.P.

Read April 4th, 1876.

A few years ago I wrote to a friend in Italy suggesting that, as he had leisure, he might try the value of the *Eucalyptus* in that country for the removal of those conditions which engender malaria. His reply was somewhat to the effect that every one was aware of the value of the tree, and everywhere trials were being made, so that there was no use in his doing what everybody was doing. This was very

cheerful, showing how rapidly the Italians had risen up to a full appreciation of new ideas. This winter I went to Rome with a desire to learn on the spot something of the conditions of the ground complained of, and receive some knowledge from the learned men of the place, as well as from any books which may not be well known here. I certainly did not obtain any confirmation of the opinion that every one was attending to the subject. I saw men in scientific and social positions in whom one might expect to find the fullest knowledge of the *Eucalyptus* and its probable value for malarious districts; but I came to the conclusion that it had excited very little, and no general, interest in Rome or the neighbouring country.

One small experiment, however, was spoken of; and several knew of it, although I met few who had seen it. It was made at the church of St. Paolo alle Tre Fontane (usually called *Tre fontane*), three or four miles from Rome; and as this is the only place I have seen from which to derive a lesson, I must be excused taking such a small example.

The church has two other churches near it, and a small residence for monks of the order of La Trappe. The station had been deserted for forty years; and the ancient building, the largest and lowest in situation, had been filled up with mud to the depth of three feet. The Campagna here is much exposed to malaria; and hence the desertion. It is by no means a dead level; neither is the upper part of the Campagna, or Agro Romano, by any means so. The soil is very deep; and it is cut into vales of a depth which I will not venture to characterize generally; suffice it to say that at this place it was from thirty to forty feet. The streams, like the Tiber itself, had brought down their mud, and the place had become, to a large extent, a desolation. In 1869 it was reinhabited; and, before entering on the dwellings,

a number of prisoners were set to clean out the whole, remove the accumulated mud, and make drains. Within the desolate spot there is now a garden; and although it is not yet in fine order, it is, at least in sunshine, a great improvement upon the neglected land around. I venture to give no exact history, but tell only such few things as were told me at the spot.

During the first season several of the monks died (I think five was the number given), and of fever such as marshes produce. A few specimens of *Eucalyptus globulus* were planted; and as they grew well, a small garden is now thinly covered over with various species. As one enters there is a peculiar odour perceptible; it is fragrant, pleasant, and resinous: some compare it to that from turpentine, some to the black currant; but every one attempts to give the name of some other odour as evidently mixed with this more prominent one. Most of the plants are two or three years old, and about ten feet high; but there is a diversity, and the oldest has been planted only five years. This largest was judged by myself and others to be about thirty-five feet high: at the height of fourteen inches from the ground it was eight inches in diameter.

Not one of the monks had died after the first year; but then the place had not been inhabited during the night until last summer. Still it will be seen that the experiment is a very small one and a very imperfect one. It is, however, important in this respect, that during the last summer and autumn (the dangerous seasons) no one had died, whilst during the first year there were several deaths. But it must be remembered that the place had been cleaned out and drained; and placed, as the buildings were, in the lower part of a valley, draining must have been much required; and the fact of mud being in the church itself, showed how

readily the whole was flooded. Although there are now some trees growing, they are only at one side of the residence, and they are small. It is true they give out a very distinct odour, very striking when one goes near, and it may be said that the fragrant oil must pervade all the surroundings even if it be not perceptible. This will certainly take place in very still weather; and the low situation protects it greatly from winds. Still, with every desire to give to the emanations of the *Eucalyptus* every virtue demanded, it is not easy to look on this as an instance of its success.

One of the monks, by name Orsise, had prepared a tincture from the leaves; and a glass of this was given to every one daily when fever showed itself. This seems to have been the really efficient agent that, in conjunction with the drainage, protected the brethren last summer.

It is not my intention to speak of this substance with details to any extent. It is, however, known that in the leaves and the bark and even in the wood of the blue gum-tree is an oil with a very strong odour. There may rather be said to be several oils; but one, which has been called eucalyptol, boils between 170° and 178° C. This is the oil which is said to resemble cajeput oil, and is said to have an effect similar to that of quinine. But the substance which has the medical effect is probably a much more volatile oil continually rising from the plant. J. Bosisto adds a volatile acid*.

The experiment mentioned shows that men may live in health in one of the worst parts of the Campagna with proper precautions, and how different things would be if this plan were general; instead of a neglected country with scarcely a house, it might be a pleasant habitation, as it

* See "Is the *Eucalyptus* a fever-destroying tree?" by J. Bosisto, Trans. & Proc. Roy. Soc. of Victoria, 1874.

once was, for many thousands. Still we should not expect this to occur if the inhabitants were obliged to take tincture of *Eucalyptus*-leaves very frequently; indeed the use of the tincture is itself an objection to the experiment. The difficulty into which Sir Samuel and Lady Baker got when detained in Africa was aggravated by fever of a marsh kind, but seems to have been entirely removed when they used the spirit from the sweet potato. The fever was not a severe one; but the fact injures to some extent the experiment at Tre Fontane.

If, however, we look to other countries, we are informed that the tree itself, with its exhalation, is quite sufficient to render a district healthy; and it is perfectly certain that if the oil is efficacious (and the evidence gives faith), those who live near must be continually taking in doses which must soon equal in amount that usually given as a cure: they must, in fact, be living in a constant vapour of this healing oil. In order to produce this condition, the houses must be surrounded with the trees; and every wind will then blow its measure of health. At Tre Fontane this is not attempted; and although more ground has been promised the monks, it is not promised to extend on every side so that they may manage to be surrounded.

In speaking of the subject, it of course occurs to ask, Is it really certain that this tree will destroy the malaria? and, in any case, will the expense be a most alarming one? Can any one venture, then, on giving advice on the subject? This last is the very point which I look on as so clear. I do not pretend to give from my own experience any information on the curative powers of the oil; but I have read enough on the subject to satisfy me that the character given to the plant is well founded. A brochure by Dr. Carlotti*,

* Assainissement des régions chaudes insalubres par Regulus Carlotti (Ajaccio, 1876).

of Corsica, goes over the whole subject very fully; and journals in abundance have treated the subject. But strong as, I believe, the arguments are, I should conclude that even if they were weak the tree ought to be encouraged and grown largely, because, if the benefit of the curative agency were less, there is another advantage arising; and that is from the nature and amount of the wood. I went to Tre Fontane with a friend, Mr. James Young, F.R.S., who plants about a million and a half trees in this country annually, and had studied the growth carefully; and he said that trees with us require from fifteen to thirty years to grow the wood made in five years by the *Eucalyptus*. The question I then asked was this—"Does this rapid growth arise from the difference of climate in Scotland and Italy?" But the surprise of the Italians at the rapid growth was equally great, although I am not able to tell the difference of increase of similar kinds of trees in the two countries. See on this also the book quoted.

M. Lambert, quoted by M. Carlotti, gives the growth in Algeria thus:—

	centim.	inches.
Circumference in centimetres and inches after 1 year	10	3·9
" " " 2 years	13	5·1
" " " 3 "	30	11·8
" " " 4 "	40	15·7
" " " 5 "	55	21·6
" " " 6 "	75	29·5
" " " 7 "	90	35·4
" " " 8 "	120	47·2
" " " 9 "	150	59·0

Mr. Carlotti has seen the growth of eleven months to be equal to 17 centimetres (=6·693 inches) in circumference.

Wood about Rome is scarce; people are cold in winter, and would gladly use a fire; and they put cloaks on even in their houses, I am told. True, their winter is short. It is a very expensive thing to have a fire in a hotel, and one

can easily burn five francs' worth of wood in a day, or at the rate of seventy pounds' worth a year for a fire in one small room. People have not fires enough for the wants of a full civilization. Even large hotels allow their fires to go out in the evening; and cooking at unusual times cannot be accomplished. The wood of a gum-tree five years old is larger than necessary for firewood, and would at any rate be of a most convenient size for splitting up. It is too large near the ground to be used entire, and much larger than any of the wood which I got in Rome or Florence.

Let us imagine this as a source of fuel, a portion of the Campagna, Maremma, Pontine marshes, &c. growing such a crop of combustible matter every five years. Where else will be found a coal-field equal in value? Let the power obtainable from this be calculated, and the value for national industry be estimated.

Professor Boyd Dawkins tells me that Sir Charles Nicholson, late Governor of Queensland, had proposed planting the gum-trees in Italy twenty years ago, and had sent seed enough to have covered the Maremma with trees; but we see what has been done.

Speaking with a Roman senator on the subject, he said it would take thirty years to grow the trees, and man was a shadow and could not look so far. I know that man is mortal, but have often heard that Rome is eternal; and it is for the future we must act. This senator had the same idea we have here, of thirty years for a pretty strong stem; but now a valuable stem, such as is used with us as-props in coal-pits, is obtained in five with apparent certainty. These five years themselves may be reduced, so far as general usefulness is concerned, because at two years the trees give out rich odour, and perhaps when still younger; and thus they begin to work almost at once, preparing wholesome habitations. The evidence seems to be that in the

second year the homesteads would, with care, be habitable, and in five years there would be a rich crop of wood off the plantation, whilst the ground cultivated for other purposes would be richer of course, because it would be receiving that attention which for a long time it never has had. The trial in this point of view ceases to be a speculation; it is a fine opening for producing wealth and comfort. Even if the malaria were not removed, the gain from the wood would be great.

It is said that the tree will not grow if the temperature falls below -4° C., or about 25° Fahr. One specimen had lost the top leaves by frost; but the great fact remaias that they lasted the five years required. There are many varieties of the tree; and some grow in Australia at the height of 4,000 feet: but I cannot learn if these produce as much wood or oil as the *E. globulus*; if they do, they promise to be valuable in this country. The *Eucalyptus viminalis* was said to be the best for drying up swamps; and it had the appearance of a willow; but it was by no means so full of oil, to all appearance, or so fragrant as the *Eucalyptus globulus*. The *Eucalyptus coccifera* never suffered at all from the frost at Tre Fontane; but it had not been long tried; still the attempt was made in its tenderest years.

M. Bosisto says that the *Eucalyptus amygdalina* grows 350 feet on high undulating forest-land, but not above 100 miles from the coast. This may be the best species for Italy. M. Carlotti seems to recommend the *E. globulus*; M. Bosisto's remarks seems to favour the *E. amygdalina*.

The conclusion arrived at was that the frost of the Campagna would not hurt to any great extent any species of the tree tried there, and the principal specimen especially. Nearer the Sabine hills it was said not to grow so well; but

there is abundant low land where it is required, and where it will grow.

The value claimed for the *Eucalyptus* is intelligible if we consider it as continually giving out a medical agent. So far as we know, this medical agent, when constantly inhaled, has no unfavourable effect—a result not very usual; but it is difficult to compare it with others, because in no case can we take medicine in doses so minute, constant, and equable. There is room here for a very interesting inquiry.

One virtue claimed for the tree is not so clear—namely, its drying up of swamps. This has been accounted for by its wonderful rapidity of growth; but I do not hear that it requires wet ground to grow upon, and am told that it grows in Australia in very dry places.

M. Bosisto mentions cool refreshing draughts of water obtained from the trunk of the *dwarf trees* in the region of the river Murray. One would be glad to hear more on this point. These dwarfs are only 25 feet high.

A paper read before the Royal Society of Victoria by J. Bosisto shows a great variation in the amount of oil. He says the *E. odorata* yields 7 fluid ounces from 2000 lb. of leaves attached to small branchlets.

viminalis	the same.
rostrata	15 ounces.
obliqua	80 „
globulus	120 „
sideroxylon	160 „
oleosa	200 „
amydalina	500 „

I suppose these numbers to represent the total amount of oils and resins.

In a report on the strength of wood, published by the Science-and-Art Department, Kensington, the *Eucalyptus*

stands high; but as there are several species from several situations, the numbers vary considerably.

In a list where the highest breaking weight is 11158 lb. in the case of white or pale iron bark, the blue gum of New S. Wales is 7364 to 6860, of Queensland 5663, 4074, 3416; and as a means of comparison may be given Russian Larch 2142, White Cedar, Queensland, 2105; the blue gum, therefore takes a high position.

That drying is an important element in the removal of the causes of malaria cannot be doubted in most cases; but it is by no means apparent in all. It cannot be doubted that Garibaldi's plan of having another passage for the Tiber south of Rome would be of great advantage, but especially to the city itself, which is so frequently inundated, and which must therefore be on a foundation exceptionally wet. The mud brought down is proverbially great; but, so far as I could judge, it was not so fine as that of the Nile, which, however, I have seen only in bottles. The Egyptian mud causes no fevers, perhaps because it is more rapidly dried up. The mud of the Tiber is deposited on a very deep soil; but the same occurs on the Nile. The depth of mud, however, may have an important bearing in a less-burning sun; but I cannot see how it affects the question of malaria.

The mud of the Tiber seems always to have been remarkable; and one wonders why it should be able to go on so long unchanged. But on going up the river the difficulty ceases; there we find mud hills, many of them conical, all with steep sides, and nearly all without any vegetation except below in some cases. There is in fact from the water an enormous deposit raised to a great depth, and enough to keep the Tiber as muddy as ever for ages. It seemed to me that all this fine earth would be very valuable in much of our country, where we are glad of a few inches over chalk, or

of a little soil scattered among large stones. Here it lies in great excess, cut into millions of little valleys, a fine study for those who wish to examine the action of streams, but a land wasted and almost useless. If these were made to bring out crops, there is room enough for a great population to live; and if crops were closely grown, the mud would proportionately cease to flow. There is a good deal of friendly interchange of favours to be made between the Lower and Upper Tiber. That this is possible seems to be proved from some of these hills being covered, although there are few in this condition. It may be difficult to coat the steep sides with any vegetation; but when the top is covered this difficulty diminishes. Still this is work for many men and for a long time.

A peculiar feature of much of the unwholesome land struck me. It began exactly at the foot of the hills. There was no gradual slope; but you came down from the hill and were at once on the plain stretching out for miles. This, of course, favours violent floods, and may be coexistent with very bad drainage of the plain. Still, as it has been remarked, the Campagna round Rome is not a plain surface, neither are the lower parts the least healthy.

I hope to think more fully of this if I describe the various opinions on the causes of malaria; at present it comes only incidentally. The great engineering works which only governments can undertake promise to be of value; but the planting to which I specially allude, seems to point out work for private men, especially land-owners. This view of the subject, if correct, must be most comforting to Italy, because it prevents that delay which is needful before great numbers can be brought to act in concert. If enough can be done privately to enable the agricultural labourers to inhabit the plains without fear, the larger schemes will come in time.

Of the general question and the inquiries of Dr. Balestra and others I hope to speak after some time. At present I shall content myself with a few recommendations.

The conclusion to which I come is, that enough is known to prove that great benefit may be expected from the cultivation of the *Eucalyptus globulus* and, perhaps more especially, the *E. amygdalina*, with others.

The best plan would probably be to begin by surrounding with trees any house to be inhabited in a district subject to malaria.

The benefit seems to arise in the second year, as soon as the plants give out a great deal of odour.

The experiment near Rome is very small and somewhat mixed, but is nevertheless enough to give great hope to those who have an opportunity to try the same in the Campagna and elsewhere.

The experiments made in other countries seem to indicate a certainty of benefit.

The value of wood being great near Rome, the *Eucalyptus* promises a substance much wanted; and there is therefore a double reason for growing it.

The growth is unusually rapid—so much so that even in five years the wood is much larger than that usually employed for burning in Rome. The expense of the crop is therefore small; and Rome is thus promised both a supply of health, of warmth, and of power by the outlay of little money with little labour. Apparently Rome need never envy us our coals; for her coals thus got will last for ever.

I have made no new discovery in relation to this subject, but have only put together a few of the more striking points, that my Roman friends may be induced to stimulate action in those who are more directly concerned, and not leave neglected such a promising source of comfort and of national wealth; for not only is this great source of power to be

obtained in the rapidly growing wood, but the removal of malaria is the introduction of better cultivation and additional wealth from other crops. *(I have followed the spelling of the several writers.)*

November 7th, 1876.

Since writing the above notice I have been told that I have underrated the work of the Romans, and that 30,000 trees of the *Eucalyptus*-species have been planted. I cannot, however, think that even that number can form an experiment of sufficient importance; and the mode of planting so as to produce a healthy house has not been described.

I must still conclude that the delay is to be regretted; and one would think that an experiment which is so promising, or, rather, certain, to some extent, would be too attractive to be neglected by any who thought upon it.—R. A. S.

VII. *On some Isomerides of Alizarine.* By Edward Schunck, Ph.D., F.R.S., and Hermann Roemer, Ph.D.

Read April 4th, 1876.

Considering the importance of every thing connected with the history of alizarine, we have been induced to undertake the study of some of the isomerides of that body. These

isomerides are interesting from a theoretical point of view, since their internal constitution is still almost unknown, and technically some of them are important from their occurring along with artificial alizarine, to the tinctorial properties of which, however, they do not contribute. The isomerides of alizarine hitherto observed are the following:—

1. *Purpuroxanthine* or *Xanthopurpurine*, a body first obtained by Schützenberger from commercial purpurine and afterwards prepared artificially by the action of reducing agents on purpurine. It crystallizes in yellow needles, soluble in alkalies with a blood-red colour.

2. *Isoalizarine*, a constituent of extract of madder, described by Rochleder, having properties very similar to those of purpuroxanthine and perhaps identical with the latter.

3. *Frangulic acid*, a substance also resembling purpuroxanthine, obtained by Faust from franguline, a constituent of the bark of *Rhamnus frangula*.

4. *Anthraflavic acid* or *Anthraflavine*, a body accompanying the artificial alizarine of commerce, first described by one of us in a paper read before this Society *, and subsequently examined by Mr. Perkin †. Its isomerism with alizarine was established by Mr. Perkin, who was the first to obtain it in a state of perfect purity.

5. *Anthraflavon*, a product obtained by the action of diluted sulphuric acid on oxybenzoic acid. The account of this body given by its discoverers, Barth and Sennhofer, shows that in most of its properties it closely resembles anthraflavine.

6. *Quinizarine*, obtained by Grimm by the action of phthalic acid on hydroquinone. Of all the isomerides it most resembles alizarine itself. Its alkaline solutions have a

* Memoirs, 3rd Series, vol. v. p. 227.

† Journ. Chem. Soc. xxiv. p. 1109, and xxvi. p. 19.

colour not differing much from that of alizarine; and it dyes mordants, while the other isomerides have no tinctorial properties.

7. *Chrysazine*, a body derived from hydrochrysammide, and carefully examined by its discoverers, Liebermann and Giesel. Acted on with strong nitric acid it yields chrysammic acid, the nitro-acid first obtained from aloes by one of us many years ago.

To these we have now to add :—

8. *Isoanthraflavine*, a substance occurring in commercial alizarine along with anthraflavine, which it closely resembles.

Chrysophanic acid, the crystalline colouring matter of rhubarb, which at one time occupied a place in the list, has been erased, since it has been shown by Liebermann that it is in reality a homologue of alizarine, having the formula $C_{15}H_{10}O_4$, and is derived not from anthracene, but from a methylanthracene.

We propose in this paper to give an account of some experiments on two of these isomerides, viz. anthraflavine and isoanthraflavine.

Anthraflavine.

We have little to add to the description of this substance given in the papers just referred to. Its melting-point is above 330° C. It is less soluble in glacial acetic acid than in alcohol. The analysis of a carefully purified specimen of the substance yielded the following numbers :—

0·2412 grm. of substance gave 0·6178 grm. CO_2 and 0·0802 grm. H_2O.

	Theory.		Experiment.
C_{14}	168	70·00	69·85
H_8	8	3·33	3·69
O_4	64	26·67	
	240	100·00	

These numbers agree with those which were obtained by Mr. Perkin, and which led him to infer that anthraflavine and alizarine are isomeric.

Barium anthraflavate, a salt which has been described in the papers referred to, loses, when dried over sulphuric acid, a considerable quantity of water, becoming at the same time much lighter in colour, and losing much of its lustre. On being now heated at a temperature of 150°–180° C., it loses a further quantity of water, and the salt has now a composition corresponding with the formula $C_{14}H_6BaO_4$, as the following numbers will show:—

I. 0·2620 grm. of substance gave 0·4284 grm. CO_2 and 0·0510 grm. H_2O.

II. 0·3540 grm. of substance gave 0·5810 grm. CO_2 and 0·0752 grm. H_2O.

III. 0·3780 grm. of substance gave 0·6150 grm. CO_2 and 0·0730 grm. H_2O.

IV. 0·3596 grm. of substance gave 0·2190 grm. $BaSO_4$.

V. 0·2646 grm. of substance gave 0·1630 grm. $BaSO_4$.

	Theory.		I.	II.	III.	IV.	V.
C_{14}	168	44·80	44·59	44·76	44·37		
H_6	6	1·60	2·16	2·36	2·14		
Ba	137	36·53				35·78	36·22
O_4	64	17·07					
	375	100·00					

The quantity of water lost by the salt when dried at 170°, after standing for some time under a bell jar with sulphuric acid, corresponds with two molecules, since 0·7710 grm. of substance lost 0·0360 grm. water or 4·66 p. c., the formula $C_{14}H_6BaO_4.2H_2O$ requiring 4·58 p. c.

Our results do not quite agree with those of Mr. Perkin, who gives for the salt dried *in vacuo*, or at 100°, the formula $2C_{14}H_6BaO_4.3H_2O$, and for the same after drying at 150°–180° the formula $2C_{14}H_6BaO_4.H_2O$.

Barium anthraflavate suspended in water is completely decomposed by a current of carbonic acid; and it is also decomposed, though not so rapidly, when exposed to the action of the atmosphere.

Tetrabromanthraflavine, $C_{14}H_4Br_4O_4$. When an excess of bromine is added to an alcoholic solution of anthraflavine the substitution-product is deposited at once in yellow crystalline needles, which are almost insoluble in the usual menstrua, such as alcohol, benzol, and glacial acetic acid.

A determination of the C and H of the substance was made, and yielded the following results:—

0·3390 grm. of substance dried at 150° gave 0·3840 grm. CO_2 and 0·0334 grm. H_2O.

	Theory.		Experiment.
C_{14}	168	30·22	30·89
H_4	4	0·72	1·09
Br_4	320	57·55	
O_4	64	11·51	
	556	100·00	

A current of chlorine gas having been passed through an alcoholic solution of anthraflavine, nothing was deposited. The solution left, on evaporation, a brown amorphous residue, insoluble in water, but easily soluble in alcohol, which was not further examined.

Nitroanthraflavine is prepared by dissolving anthraflavine in fuming nitric acid, and, after allowing the solution to stand some time, adding water, which precipitates the nitro-acid as a light-yellow crystalline powder. It is obtained on spontaneous evaporation of its alcoholic solution in large well-defined rhombic crystals of a deep yellow colour. Most of its salts, such as the potassium, sodium, magnesium, barium, silver, and mercury salts, are soluble in boiling water, and crystallize in lustrous needles varying in colour from light yellow to brownish red. From a deter-

mination made of the C and H we infer that its formula is $C_{14}H_4(NO_2)_4O_4$, and, therefore, that it is a tetranitroanthraflavine.

0·3126 grm. of substance gave 0·4554 grm. CO_2 and 0·0344 grm. H_2O.

	Theory.		Experiment.
C_{14}	168	40·00	39·73
H_4	4	0·95	1·22
N_4	56	13·33	
O_{12}	192	45·72	
	420	100·00	

Nitroanthraflavine appears, therefore, to be isomeric with nitrochrysazine, and, therefore, also with chrysammic acid, which, according to Liebermann, is identical with nitrochrysazine.

By treatment with tin and hydrochloric acid, nitroanthraflavine yields a dark blue powder, which is almost insoluble in alcohol and glacial acetic acid, but dissolves in caustic alkali with a fine violet colour like that of an alkaline solution of alizarine. The solution on exposure to the air soon assumes a brownish purple colour.

Diacetylanthraflavine has already been described by Perkin. We found its melting-point to be at 227°.

Diethylanthraflavine, $C_{14}H_6(C_2H_5)_2O_4$.—This compound was prepared by heating a mixture of anthraflavine, caustic soda, and iodide of ethyl, to which a little alcohol was added, in sealed tubes to 120°, and crystallizing the product from boiling alcohol. It crystallizes in light-yellow needles, which are soluble in benzol and glacial acetic acid, but insoluble in water. It melts at 232°. The fused substance on again solidifying is converted into a mass of prismatic crystals. Diethylanthraflavine dissolves without change in concentrated sulphuric acid, forming a red solution, the spectrum of which shows a well-defined absorp-

tion-band on the confines of the green and blue. It is insoluble in dilute caustic potash lye.

Analysis yielded the following results :—

0·2382 grm. of substance dried at 120° gave 0·6352 grm. CO_2 and 0·1166 grm. H_2O.

		Theory.	Experiment.
C_{18}	216	72·97	72·72
H_{16}	16	5·41	5·43
O_4	64	21·62	
	296	100·00	

Dimethylanthraflavine was prepared by the same method as diethylanthraflavine, from which it can hardly be distinguished. It melts at 247°–248°.

Isoanthraflavine.

This isomeride of alizarine and anthraflavine was obtained from a by-product of the manufacture of alizarine, which Mr. Perkin kindly presented to one of us some time ago for purposes of investigation. According to Mr. Perkin, this product had been obtained by treating crude alizarine with lime-water, filtering the red extract and collecting the precipitate produced on adding acid.

The product was treated with dilute caustic soda lye, in order to separate some anthraquinone which was present. The filtrate gave with hydrochloric acid a yellow flocculent precipitate, which was filtered off and treated with cold baryta-water until nothing more dissolved. The residue left undissolved after this treatment consisted of barium anthraflavate, and was employed for the preparation of anthraflavine. The blood-red solution gave on the addition of acid a yellow precipitate consisting of isoanthraflavine. This was purified by solution in boiling alcohol, from which it crystallized in long needles of a yellow or brownish yellow

colour. Several crystallizations from alcohol were requisite to render it quite pure. Sometimes it yielded golden-yellow micaceous scales; but these on recrystallization always gave needles. The needles contain water of crystallization, which is driven off by heating to 100°, the crystals losing their lustre by drying. The scales seem to be anhydrous.

The properties of isoanthraflavine are very similar to those of anthraflavine. Its melting-point is above 330°. When slowly heated between watch-glasses, it yields a sublimate consisting of star-shaped groups of yellow needles, a carbonaceous residue being left, which is more considerable than in the case of anthraflavine.

It is a little more soluble in boiling water than anthraflavine, the solution being at first yellow, but acquiring a reddish tint on continued boiling. It dissolves in concentrated sulphuric acid in the cold, and in alcohol and ether on boiling; but it is almost insoluble in benzol and chloroform. It dissolves in caustic alkalies with a cherry-red colour, and may thus be easily distinguished from anthraflavine, the alkaline solutions of which are yellow or, when concentrated, reddish yellow. In concentrated sulphuric acid isoanthraflavine dissolves with a cherry-red, anthraflavine with a yellow colour. The two substances may also be readily distinguished by their behaviour towards lime- and baryta-water, in which isoanthraflavine dissolves even in the cold, yielding red solutions, whilst anthraflavine only dissolves in boiling baryta-water, and is almost insoluble in lime-water at all temperatures. An alcoholic solution of isoanthraflavine gives no precipitate with an alcoholic solution of acetate of lead. Isoanthraflavine imparts no colour whatever to mordants; and its behaviour in this respect is the same as that of most of the other isomerides of alizarine. In some of its properties it strongly resembles purpuroxanthine; but having prepared a specimen of the latter

according to Schützenberger's process, we are enabled to assert positively that the two bodies are not identical.

The water of crystallization of the isoanthraflavine crystallizing in needles was determined by heating the substance after drying over sulphuric acid to 150°.

I. 0·2150 grm. of substance treated in this way lost 0·0146 grm. H_2O, or 6·79 p. c.

II. 0·2064 grm. of substance lost 0·0138 grm. H_2O, or 6·69 p. c.

The analysis of the dry substance yielded the following results :—

I. 0·2700 grm. of substance gave 0·6910 grm. CO_2 and 0·0880 grm. H_2O.

II. 0·2808 grm. of substance gave 0·7154 grm. CO_2 and 0·0880 grm. H_2O.

III. 0·2622 grm. of substance gave 0·6720 grm. CO_2 and 0·0876 grm. H_2O.

IV. 0·2484 grm. of substance gave 0·6362 grm. CO_2 and 0·0840 grm. H_2O.

V. 0·2692 grm. of substance gave 0·6904 grm. CO_2 and 0·0908 grm. H_2O.

	Theory.		I.	II.	III.	IV.	V.
C_{14}	168	70·00	69·79	69·48	69·89	69·85	69·94
H_8	8	3·33	3·62	3·48	3·71	3·75	3·74
O_4	64	26·67					
	240	100·00					

The determinations of the water of crystallization given above lead to the formula $C_{14}H_8O_4.H_2O$ for the crystallized acid, which requires 6·97 p. c. for the quantity of water expelled on heating.

The barium compound of isoanthraflavine crystallizes in small dark red needles resembling barium anthraflavate.

It contains water of crystallization, which it loses on being heated to 150°.

0·1810 grm. barium isoanthraflavate dried at 150° gave 0·1104 grm. $BaSO_4$, or 35·86 p. c. of barium, and 0·1146 grm., or 63·31 p. c. isoanthraflavine. The formula $C_{14}H_6BaO_4$ requires 36·53 p. c. Ba, and 64·00 p. c. isoanthraflavine.

The derivatives of isoanthraflavine, as far as we have examined them, closely resemble those of anthraflavine.

Tetrabromisoanthraflavine, $C_{14}H_4Br_4O_4$. — This compound was prepared in the same way as tetrabromanthraflavine. It crystallizes in yellow needles, which are easily soluble in glacial acetic acid, but dissolve with difficulty in boiling alcohol. Analysis gave the following results:—

I. 0·3712 grm. of substance dried at 150° gave 0·4086 grm. CO_2 and 0·0396 grm. H_2O.

II. 0·1736 grm. of substance gave 0·2344 grm. AgBr.

	Theory.		I.	II.
C_{14}	168	30·22	30·02	
H_4	4	0·72	1·18	
Br_4	320	57·55		57·45
O_4	64	11·51		
	556	100·00		

Diacetylisoanthraflavine, $C_{14}H_6(C_2H_3O)_2O_4$. — Isoanthraflavine and acetic anhydride, heated together, at 160°–180° yield a product crystallizing in light yellow microscopic needles, soluble in alcohol and easily soluble in glacial acetic acid. The substance commences to soften at 175°, and melts completely at about 195°. It is decomposed by alcoholic potash solution. That it contains 2 molecules of acetyl was proved by the following determination of the quantity of isoanthraflavine obtained by decomposition with alcoholic potash.

0·1370 grm. of substance gave 0·1010 grm. $C_{14}H_8O_4$ or 73·72 p. c., the calculated amount being 74·07 p. c.

Diethylisoanthraflavine, $C_{14}H_6(C_2H_5)_2O_4$.—This compound was prepared in the same way as diethylanthraflavine, which it closely resembles. It crystallizes from alcohol in long, light-yellow, shining needles. Glacial acetic acid and benzol dissolve it more readily than alcohol or ether. It melts at 193°–194°. It dissolves in concentrated sulphuric acid, forming a purple solution, the spectrum of which shows two faint absorption-bands—one in the yellow, and the other in the green. The analysis of the substance led to the following results:—

0·1814 grm. of substance dried at 120° gave 0·4824 grm. CO_2 and 0·0890 grm. H_2O.

		Theory.	Experiment.
C_{18}	216	72·97	72·52
H_{16}	16	5·41	5·45
O_4	64	21·62	
	296	100·00	

By treatment with fuming nitric acid isoanthraflavine yields a nitro-acid resembling nitro-anthraflavine; but want of material has prevented our examining it minutely.

We will conclude with a few remarks on the action of caustic alkalies on anthraflavine and isoanthraflavine. On a former occasion it was stated by one of us that anthraflavine, acted on by fusing potassium hydrate, yields alizarine; and it was this supposed convertibility into alizarine which led Liebermann and others to the conclusion that anthraflavine was identical with monoxyanthraquinone.

We are now in a position to assert confidently that the product of the action is not alizarine. On repeating the experiment with a larger quantity of anthraflavine, we ob-

tained a substance which, after purification, crystallized from alcohol in small needles strongly resembling, but certainly not identical with alizarine. It is probably an isomeride of purpurine. Isoanthraflavine, when treated in the same way, yields a body which has all the properties of Mr. Perkin's anthrapurpurine.

VIII. *On the Manner in which Raindrops and Hailstones are formed.* By Professor Osborne Reynolds, M.A.

Read October 31st, 1876.

When the particles of water or ice which constitute a cloud or fog are all of the same size, and the air in which they are sustained is at rest or is moving uniformly in one direction, then these particles can have no motion relatively to each other. The weight of the particles will cause them to descend through the air with velocities which depend on their diameters; and since they are all of the same size, they will all move with the same velocity.

Under these circumstances, therefore, the particles will not traverse the spaces which separate them, and there can be no aggregation so as to form raindrops or hailstones.

If, however, from circumstances to be presently considered, some of the particles of the cloud or fog attain a larger size than others, these will descend faster than the others, and will consequently overtake those immediately beneath them; with these they may combine so as to form still

larger particles, which will move with greater velocity and, more quickly overtaking the particles in front of them, will add to their size at an increasing rate.

Under such circumstances, therefore, the cloud would be converted into rain or hail, according as the particles were water or ice.

The size of the drops from such a cloud would depend simply on the quantity of water suspended in the space swept through by the drop in its descent—that is to say, on the density and thickness of the cloud below the point from which the drop started.

My object in this paper is to suggest that this is the actual way in which raindrops and hailstones are formed. I was first led to this conclusion from observing closely the structure of ordinary hailstones.

Although to the casual observer hailstones may appear to have no particular shape except that of more or less imperfect spheres, on closer inspection they are seen all to partake more or less of a conical form with a rounded base like a sector of a sphere.

Fig. 1.

Perfect Hailstone.

In texture they have the appearance of an aggregation

of minute particles of ice fitting closely together, but without any crystallization such as that seen in the snow-flake—although the surface of the cone is striated, the striæ radiating from the vertex.

Such a form and texture as this is exactly what would result if the stones were formed in the manner described above. When a particle which ultimately formed the vertex of the cone, started on its downward descent and encountered other particles on its lower face, they would adhere to it, however slightly. The mass, therefore, would grow in thickness downwards; and as some of the particles would strike the face so close to the edge that they would overhang, the lower face would continually grow broader, and a conical form be given to the mass above.

When found on the ground the hailstones are generally imperfect; and besides such bruises as may be ascribed to the fall, many of them appear to have been imperfect before reaching the ground. Such deformities, however, may be easily accounted for.

The larger stones fall faster than those which are smaller, and consequently may overtake them in their descent; and then the smaller stones will stick to the larger and at once deform them. But besides the deformation caused by the presence of the smaller stone, the effect of the impact may be to impart a rotary motion to the stone, so that now it will no longer continue to grow in the same manner as before. Hence we have causes for almost any irregularities of form in the ordinary hailstone.

It appears from the numerous accounts which have been published, that occasionally hailstones are found whose form is altogether different from that described above. These, however, are exceptional; and to whatever causes they may owe their peculiarities, these causes cannot affect the stones to which I am referring.

Again, on careful examination, it is seen that the ordinary hailstones are denser and firmer towards their bases or spherical sides than near the vertex of the cone, which latter often appears to have broken off in the descent. This also is exactly what would result from the manner of formation described above.

Fig. 2.

Broken Hailstone.

When the particle first starts, it will be moving slowly, and the force with which the particles impinge upon it will be slight and, consequently, its texture loose; as, however, it grows in size and its velocity increases it will strike the particles it overtakes with greater force, and so drive them into a more compact mass. If the velocity were sufficient, the particles would strike with sufficient force to adhere as solid ice; and this appears to be the case when the stones become large—as large as a walnut, for instance.

An idea of the effect of the suspended particles on being overtaken by the stone, may be formed from the action of the particles of sand in Mr. Tilghman's sand-blast, used for cutting glass. The two cases are essentially the same, the only difference being that the hailstone is moving

through the air, whereas in the case of the sand-blast the object which corresponds to the stone is fixed, and the sand is blown against it.

By this sand-blast the finest particles of sand are made to indent the hardest material, such as quartz or hard steel; so that the actual intensity of the pressure between the surface of the particles of sand and that of the object they strike must be enormous. And yet the velocity of the blast is not so much greater than that at which a good-sized hailstone descends. It is easy to conceive, therefore, that the force of the impact of the suspended particles of ice, if not much below the temperature of freezing, on a large hailstone, would drive them together so as to form solid ice; for the effect of squeezing two particles of ice together is to cause them to thaw at the surface of contact, and as soon as the pressure is relieved they freeze again; and hence their adhesion.

Nor does there appear to be any other way in which these ordinary hailstones can be formed. They are clearly not raindrops frozen, or they would be somewhat transparent; neither are they aggregations of snow crystals. Nor can they be formed by the condensation and refrigeration of vapour on a nucleus of ice; for there is no way of getting rid of the heat which must be developed by such a process: the heat developed by the condensation of vapour one seventh of the weight of the stone would be sufficient to thaw the entire stone.

The hailstones are clearly aggregations of small frozen particles such as those which form a cloud. Nor is it possible that they can have been drawn together by some electrical attraction; for whatever such attraction we can conceive, it will not explain the conical shape of the stones or their increase in density towards their thicker sides. These clearly show that the particles have aggregated from

one direction, and with an increasing force as the size of the stone has increased.

It appears as though it might be possible to make artificial hailstones. If a stream of frozen fog were driven against any small object, then the frozen particles should accumulate on the object in a mass resembling a hailstone. Not seeing my way to obtain such a stream of frozen fog, I thought it might be worth while to try the effect of blowing very finely powdered plaster of Paris. I therefore introduced a stream of this material into a jet of steam issuing freely into the air (which I hoped would moisten the powdered plaster sufficiently to cause it to set firmly in whatever form it collected into). The jet was directed against a splinter of wood.

In this way I obtained masses of plaster very closely resembling hailstones. They were all more or less conical, with their bases facing the jet. But as might be expected, the angles of the cones were all smaller than those of the hailstones. Two of their figures are shown in the sketches annexed (p. 54).

The striæ were strongly marked, and exactly resembled those of the hailstone. The bases also were rounded. They were somewhat steeper than those of the hailstone; but this was clearly due to the want of sufficient cohesive power on the part of the plaster: it was not sufficiently wet. Owing to this cause also it was not possible to preserve the lumps when they were formed, as the least shake caused them to tumble in pieces.

I also tried a jet of the vapour of naphthaline, which at ordinary temperatures is solid, driven by means of a cross blast of air against a small object; and in this way I obtained masses closely resembling hailstones: but these also were too fragile to bear moving. At ordinary temperatures the powdered naphthaline does not adhere like ice when pressed

into a lump. No doubt at very low temperatures ice would behave in the same way; that is to say, the particles would not adhere from the force of impact. Hence it would seem probable that for hailstones to be formed the temperature of the cloud must not be much below freezing-point.

Fig. 3.

Fig. 4.

Imitations in Plaster of Paris.

That the temperature of the cloud exercises great influence on the character of the hailstones cannot be doubted; and if, as has been suggested by M. L. Dufour, the particles will sometimes remain fluid, even when the temperature is as low as 0° F., it is clear that as they are swept up by a falling stone they may freeze into homogeneous ice, either in a laminated or crystalline form. Upon these questions, however, I do not wish to enter, as they have no bearing on the question as to the manner in which the mass of the stone is accumulated; and I only mention them to show that, if there are unexplained peculiarities, there are also causes the effects of which have not as yet been fully considered.

This view of the manner in which hailstones are formed at once suggests that raindrops may be formed in the same way; nor does there appear, on further consideration, to be any reason to suppose that such is not the case.

Of course a raindrop shows none of the structural peculiarities of the hailstone; and consequently we have not the same evidence of the manner in which raindrops are formed; but the explanation is sufficient, and there is apparently no other.

Raindrops cannot possibly have grown to the size with which they reach the earth by the condensation of the vapour of the air which they pass through, for the same simple reason as that just stated for hailstones, namely that there is no way in which the heat developed by condensation can be got rid of. The fact that the upper regions of the air from which the drops start are colder than those through which they descend, might, as has been supposed, cause the drop to grow by condensing vapour in the air through which it passes—but, as was shown by Mr. Baxendell*, only to a very small extent, and one the limit of which may be easily estimated.

* Memoirs of the Lit. & Phil. Soc. of Manchester, vol. i. 3rd Series, p. 399.

Suppose the drop to start having a weight w_1 and a temperature t_1, and on reaching the earth to have a temperature t_2. Then the increase in the quantity of heat in the drop would be $(t_2-t_1)w_1$ nearly. This heat would be developed by the condensation of a weight of water $(t_2-t_1)\frac{w_1}{1000}$ nearly; so that, even supposing $t_2-t_1 = 100°$ F., which it could not possibly be, the increase in the weight of the drop could not be one tenth.

It is obvious also that the drop would not have parted with its heat to the air it passes through; for it is assumed to be colder than this air. Therefore the only way in which it could have parted with its heat would have been by radiation. Some heat might be lost in this way, but only a very small amount, and one of which an approximate estimate may be made. For after the drop had acquired a considerable size, say one hundredth of a foot in diameter, the time occupied in its descent would be very small. Assume this to be one minute; and assume that during this time the drop is 100 degrees hotter than the surrounding objects, although this is of course far beyond what could possibly be. According to the most accurate data the amount of heat it would then lose would not be sufficient to condense $\frac{1}{200}$ of a grain of water*—an altogether

* The surface of a drop whose diameter is ·05 ft. is ·000314 sq. ft. Now if the temperature of the surrounding objects be zero Centigrade, and the temperature of the drop be 60° Centigrade, then, assuming the radiation from the surface of the drop to be the same as the radiation from the surface of glass, we have (see Balfour Stewart on Heat, p. 228):—

$$R = 10(1{\cdot}600770 - 1)A,$$

where R is the heat radiated from the surface A in one minute, the units being the heat required to raise 1000 grains of water 1° C. This gives

$$R = 6A$$

or $$R = {\cdot}002 \text{ nearly.}$$

Now if R′ be the same quantity of heat, the unit being the heat required to raise one grain 1° Fahr., $R' = 1800\ R$,

$$= 3{\cdot}6.$$

inappreciable amount when compared with the weight of the drop, which would be nearly the quarter of a grain.

It appears clear, therefore, that the only way in which a falling drop can grow is by the aggregation to itself of the particles of moisture in the air; and the only way in which it can encounter these is by its downward motion through this air.

Such a means of growth is amply sufficient to account for the size of raindrops or of hailstones.

If we suppose all the vapour which a body of saturated air at 60° F. would contain, over and above what it would contain at 30°, to be changed into a fog or cloud, then, if a particle, after commencing to descend, aggregated to itself all the water suspended in the volume of air through which it swept, the diameter of the drop after passing through 2000 feet* would be more than an eighth of an inch, and after passing through 4000 feet a quarter of an inch, and so on; so that in passing through 8000 feet of such cloud it would acquire a diameter of half an inch. Now, as clouds must often contain more water than what is here supposed, there is no difficulty in explaining the

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This is equivalent to the latent heat of condensation of ·0036 grain of water.

Again, let w be the weight of the drop; then

$$w = 7000 \times 62{\cdot}5 \times \tfrac{4}{3}\pi r^3$$
$$= {\cdot}21 \text{ or nearly } \tfrac{1}{5} \text{ grain.}$$

* If x be the diameter of the drop after descending a distance h, and ρ the volume of water suspended in a unit volume of air, then the increase of volume of the drop in descending a distance dh is given by

$$\frac{\pi}{2}x^2dx = \rho\frac{\pi}{4}x^2dh;$$

$$\therefore dx = \frac{\rho}{2}dh;$$

or

$$x = \frac{\rho}{2}h.$$

Hence, if $\rho = {\cdot}00001$, $x = {\cdot}000005h$;

and if $x = {\cdot}01$, $h = 2000$ feet.

size of the drops. The difficulty is rather the other way, in explaining why the drops are not sometimes larger than they are.

There are, however, two reasons why raindrops do not acquire the full size which might be expected on the above assumptions.

In the first place, the drop will not aggregate to itself all the particles in front of it. Some of these will be swept away sideways by the diverging current of air; and the smaller the particles are the more will this be the case. This is, of course, true for hail as well as for rain.

The second reason applies only to rain, and explains why it is that hailstones sometimes acquire magnitudes never approached by raindrops.

A drop retains its form simply by the surface-tension of the water; and as this is the same whatever may be the size of the drop, its power to hold the drop together diminishes as the size of the drop increases, whereas the velocity and consequent tendency of the air to disturb the shape of the drop increase with its size. Hence it must eventually arrive at such a size that it can no longer hold together, but will be blown to pieces by the rush of air past it. This action may be seen in a waterfall or a fountain, where, in passing through the air, a solid column of water is separated into drops not larger than large raindrops.

The same reasoning does not hold for hailstones, which are held together by the adhesion of the particles throughout their entire mass, and whose compactness and strength increase with their size. It is, however, the case that the smaller end of the stone, where the texture is looser, appears to be blown off in its subsequent descent, especially when the stones acquire a larger size.

It seems, therefore, that, so far as the growth of a drop or a stone is concerned, the particles it overtakes in its

downward path are a necessary and sufficient cause; but the origin of the drops and stones requires further explanation. Why should some of the particles in a cloud be larger than the others, as it is necessary for them to be in order that they may commence a more rapid descent?

A cloud does not always rain; and hence it would seem that in their normal condition the particles of a cloud are all of the same size and have no internal motion, and that the variation of size is due to some irregularity or disturbance in the cloud.

Such irregularity would result when a cloud is cooling by radiation from its upper surface. The particles on the top of the cloud being more exposed would radiate faster than those below them; and hence they would condense more vapour and grow more rapidly in size. They would therefore descend and leave other particles to form the top of the cloud. In this way we should have in embryo a continuous succession of drops.

Eddies in the cloud also form another possible cause of the origin of drops and stones. Whenever the direction of motion of a portion of the cloud is not straight, the suspended particles will have more or less motion through the air. And if, as in an eddy, the motion of the cloud varies from point to point both in direction and magnitude, then the motion of the particles through the air will also vary, and they may overtake one another and, combining, form larger particles or drops in embryo.

Whatever may be the cause of the variation in the size of the particles which form the cloud, we may know from observations on fogs that such variations do exist. In fogs we have particles of all sizes, from those which are too fine to be seen even by the aid of a microscope, and which will remain suspended for hours without any appreciable descent, up to such a size that they can be easily detected

with the naked eye, and descend with a very appreciable velocity so as to form a drizzle. When a coarse mist, such as this, is superimposed over a fine mist, then rain must ensue if the particles are water, and hail if they are ice.

Although, as has been shown, a raindrop cannot add considerably to its volume by condensing the vapour from the air through which it passes; the reverse of this is not the case. The raindrop may be diminished by evaporation. Whenever a raindrop falls through dry air (that is, air of which the dew-point is below the temperature), evaporation might, and would, go on to almost any extent, and the size of the drops be diminished until they entirely vanished, the heat for evaporation being supplied from the air, which would be warmer than the drop.

The case of snow differs from that of hail. The snow crystals are clearly formed by the condensation of vapour, and not by the mere aggregation of particles of ice. In this case the latent heat developed in condensation is probably dissipated by radiation, the shape and smallness of the crystals causing them to descend very slowly, and so affording time for the radiation to produce an effect.

But even in snow we see the effect of aggregation. The individual crystals never acquire a large size. But in their descent, the larger ones overtaking the smaller, they form into flakes. In this case the aggregation may be seen taking place. If when large flakes of snow are falling fast without wind, the eye be fixed on a large flake as high as it can at first be perceived, and follow this flake in its subsequent descent, it may sometimes be seen to overtake another flake and combine with it, the two descending together.

IX. *On Changes in the Rates of Mortality from different Diseases during the Twenty Years* 1854–73. By Joseph Baxendell, F.R.A.S.

Read December 26th, 1876.

It is well known to all who take an interest in questions relating to health and disease that the sanitary measures which have been carried out, at great cost, in all parts of the country during the last twenty or thirty years appear to have had no perceptible effect in improving the general state of the public health and reducing the rate of mortality; and a feeling is rapidly gaining ground that the sanitary schemes which have been imposed upon the country by parliament are either of very questionable utility or have been carried out by local authorities in a very negligent and inefficient manner, while the enormous and rapidly increasing expenditure which they involve is becoming a matter for very serious consideration, and is especially annoying and disappointing when it is seen that they have so far signally failed to produce the results which their promoters so confidently anticipated. Occasionally, when a season or succession of seasons favourable to the public health has reduced the death-rate for a time, sanguine officials have hastily and inconsiderately attributed the reduction to their so-called sanitary improvements and arrangements; but when a period is taken sufficiently long to eliminate the influence of changes in the seasons, it is found that no reduction whatever has taken place, but that, on the contrary, the rate has still a slight tendency to in-

crease. Thus the average death-rate for all England and Wales during the ten years 1854–63 was 22·14, while in the following ten years 1864–73 it was 22·45, or 1·4 per cent. greater.

That the present sanitary system is very defective and not likely to produce any material improvement in the public health is virtually admitted by many officers of health, who now frequently state in their reports that the deaths which have occurred in their districts were from causes over which the board or sanitary authority had no control; by which it is commonly understood that the diseases which caused the deaths were not contagious or infectious, or, as they are now frequently termed, "preventible," the operation of the present system being thus confessedly limited to a group of diseases which cause only about one seventh of the total number of deaths. But we have seen that during the last twenty years there has been no diminution in the general death-rate; and it is evident, therefore, either that sanitary measures have failed entirely to reduce the mortality from the so-called preventible diseases, or that any reduction they have effected has been counterbalanced by a corresponding increase in the mortality from the non-preventible class. With a view to determine which of these conclusions is the true one, I have compared the rates of mortality from different diseases during the ten years 1854–63 with those for the succeeding ten years, 1864–73, the year 1873 being the last for which an annual report has been published by the Registrar General.

The mean population of England and Wales for the first period was 19,592,113, and for the second 22,099,664, the ratio of these numbers being as 1 : 1·1280.

The following table shows the number of deaths in each period from each of eight infectious diseases, the corre-

sponding ratios, the differences between these ratios and that of the increase of population, and the percentages of increase or decrease of the death-rate:—

	Total Deaths.		Ratios.	Differences from ratio of increase of population.	Percentages of increase or decrease of death-rate.
	10 years, 1854–63.	10 years, 1864–73.			
Scarlet fever and Diphtheria	218,248	227,849	1·0439	−·0841	− 7·4
Fever, Typhus, Typhoid, and simple continued Fever	169,511	180,467	1·0646	−·0644	− 5·6
Whooping-cough	104,353	110,722	1·0610	−·0670	− 5·9
Measles	88,254	89,121	1·0098	−·1182	−10·4
Croup	52,160	47,562	0·9118	−·2162	−19·1
Small-pox	33,515	70,458	2·1023	+·9743	+86·3
Erysipelas	18,510	19,320	1·0389	−·0891	− 7·9
Influenza	13,667	5,174	0·3859	−·7495	−66·4

It appears, therefore, that of these eight infectious diseases the mortality from seven has diminished very sensibly, while that from the eighth, small-pox, shows an extraordinary increase. But this disease is the only one for which we have a special preventive; and yet, although this preventive has been almost universally applied under the powers of Acts of Parliament, and all the other infectious diseases appear to have yielded in various degrees to the influence of ordinary sanitary improvements, this disease has increased at a rate greatly exceeding that at which any other infectious disease has decreased, and even, as I shall presently show, greatly exceeded that at which any other disease whatever has increased or decreased, thus strongly suggesting the idea that special means must have been taken to encourage its development.

Thirty years ago it was commonly supposed that small-pox occurred much more frequently among children and young persons than among adults; and I find that in the five years 1848–52, the first for which we have accurate

returns, the deaths of males under five years of age were 66·5 per cent. of the total number of deaths at all ages; but in the five years 1869–73 the proportion had fallen to 31·0 per cent. For females the numbers were respectively 72·2 and 36·1 per cent. The proportion of deaths at all ages above five years was therefore more than doubled. But taking the deaths between the ages of 25 and 65, the proportion was more than trebled in the case of males, and more than quadrupled in the case of females; and taking into account the increase in the total mortality at all ages, it appears that the death-rate of males between the ages of 25 and 65 years was 4·46 times greater in 1869–73 than in 1848–52, and of females 5·14 times greater. These remarkable facts seem to indicate that this loathsome disease has in some way become very much modified and more virulent, or that some cause has been in operation which has rendered the adult portion of the population very much more susceptible to its attacks; and it is evident, therefore, that it is a much more serious evil to society than it was twenty or thirty years ago, not only because the mortality from it has greatly increased, but also because a very much larger proportion of that mortality now falls upon the producing classes. In spite of sanitary improvements and the protection supposed to be afforded by the application of a special preventive, the death-rate from it has increased at all ages without exception, but more especially at those ages when it is most important for the general interests of society that life should be saved.

The general conditions which tend to produce an outbreak of small-pox also favour the development of scarlet fever; so that if a marked increase takes place in one, an equally marked increase may soon be expected to appear in the other; and yet, while scarlet fever has been favourably influenced by ordinary means and treatment, small-pox

has not received the slightest check, but, on the contrary, has gone on increasing in fatality in spite of the application of both ordinary and extraordinary means for its suppression*.

The results given above seem to indicate clearly that the infectious diseases, as a class, have, so far, been favourably acted upon by the carrying-out of sanitary measures, and the mortality from them sensibly reduced. It follows, therefore, that the total mortality from diseases of the non-preventible class must have increased to a slightly greater extent; and it becomes a matter of considerable interest and importance to ascertain which of these diseases have contributed to this increase, and in what proportions.

An examination of the returns soon showed that considerable changes had taken place in the mortality from several of the diseases which are usually regarded as not being under the control of sanitary officials, and that, while in some it had increased, in others it had diminished, though, on the whole, to a somewhat less extent.

The principal non-preventible diseases which increased in fatality, and the rates of increase, were as follows :—

	Per cent.		Per cent.
Bronchitis	35·1	Tabes mesenterica	16·1
Heart-disease	32·3	Cephalitis	15·0
Diarrhœa	26·3	Rheumatism	13·5
Lung-disease	24·0	Brain-disease	11·4
Cancer	22·0	Apoplexy	11·1
Liver-disease	18·1	Premature birth	9·0
Kidney-disease	16·7	Paralysis	8·2

The following are the principal non-preventible diseases which have diminished in fatality, and their rates of diminution :—

* Since this was written the Registrar-General's Report for 1874 has been issued; and I find that the deaths from scarlet fever rose from 13,144 in 1873, to 24,922 in 1874. This was a clear indication that another outbreak of small-pox was at hand; and, accordingly, in the following year, 1875, the epidemic commenced from which we are now suffering.

	Per cent.		Per cent.
Dropsy	31·4	Hydrocephalus	11·3
Asthma	25·2	Old age	8·1
Enteritis	19·6	Convulsions	7·5
Scrofula	17·2	Phthisis	7·1
Pneumonia	16·6		

It has not been thought worth while, for the present at least, to include in these lists diseases which have increased or decreased in fatality less than 7 per cent., or from which the number of deaths averaged less than 2,500 per annum during the last ten years.

An analysis of the death-returns of a few of the more important of the diseases in the above lists has yielded the following results.

Bronchitis.

During the ten years 1854–63 the total number of deaths caused by this disease was 277,335; and in the following ten years it rose to 422,806. We have, then,

$$\frac{\left(\frac{422806}{277335} - 1{\cdot}1280\right)100}{1{\cdot}1280} = +35{\cdot}1.$$

The increase of the death-rate from bronchitis therefore amounted to 35·1 per cent., or to a loss of no less than 109,972 additional lives, while the total number of lives saved by the improvement in the whole class of infectious diseases amounted to only 36,995, or one third of the extra loss from bronchitis alone; so that while all the sanitary improvements of the last twenty years may be regarded as having effected a saving of one life in every 134, the increased mortality from bronchitis alone has resulted in a loss of one in every 45. This great increase in the death-rate from bronchitis is the more remarkable from the fact that it has been gradual and steady, and appears to have been but little influenced by changes in the character of

the seasons. Thus the mean death-rate during the five years

1854–58 was 1·296,
1859–63 „ 1·528,
1864–68 „ 1·778,
1869–73 „ 2·040.

Comparing the rate during the last five years with that during the first five, the increase is 57·4 per cent!

We have seen that the increase in the mortality from small-pox was relatively much less among infants and young children than among adults; but with bronchitis the reverse of this took place. Thus in the five years 1854–58 the deaths of males under one year of age were 21·2 per cent. of the total deaths at all ages, and under five years 36·3; in the five years 1869–73 the numbers were respectively 25·9 and 41·2. It is remarkable, however, that between the ages of ten and twenty-five there was a slight decrease, so that the curve representing the variations in the mortality at different ages from bronchitis has two well-marked maxima—one at ages under one year, and the other between the ages of fifty-five and sixty-five. The increase at ages under one year amounted to 49·7 per cent., and at ages between fifty-five and sixty-five it was 35·6 per cent. During the year 1873, out of every 19 deaths from all causes, two were due to bronchitis alone. In the seven years 1848–54 the proportion was two deaths from bronchitis out of every 47 from all causes.

I have already remarked that the increased mortality from bronchitis cannot be attributed to changes in the character of the seasons. It is, however, due to some cause which affects the entire population of the kingdom, both male and female, since I find that the increase has been general and not confined to smoky towns and manufacturing districts, as will be seen from the following com-

parison of the death-rates in North Wales and three of the healthiest counties in England, Westmoreland in the north, and Sussex and Dorset in the south, and in Lancashire, the most unhealthy county :—

	Mean death-rate, 1854–63.	Mean death-rate, 1864–73.	Increase per cent.	Percentage of total death-rate, 1854–63.	Percentage of total death-rate, 1864–73.
North Wales.. ...	0·690	1·092	58·2	3·32	5·13
Westmoreland ...	0·845	1·265	49·7	4·66	7·18
Sussex.....	0·945	1·269	34·2	4·96	6·85
Dorset	1·029	1·468	42·6	5·41	8·01
Lancashire.....	2·203	3·000	36·1	8·48	10·98

Heart-disease.

This disease ranks next in order after bronchitis in the rate at which its fatality has increased. In the first decade the total number of deaths was 155,850, and in the second 232,566. Allowing for the increase of population, these numbers show an increase of no less than 32·8 per cent in the death-rate. 60 per cent. of the deaths occurred between the ages of 45 and 75, the maximum rate being between the ages of 65 and 75; but the greatest increase of the rate took place between the ages of 25 and 35, and amounted to 41·5 per cent.

Diarrhœa.

The total number of deaths from this disease in the first ten years was 154,513, and in the second 220,173. These numbers give an increase of 26·3 per cent. in the death-rate. For males the increase was 27·5 per cent., and for females 25·0 per cent. The death-rate of males under one year of age increased 42·5 per cent., and all under five years 36·0 per cent.; but at five years and all above it diminished 7·1 per cent.

Cancer.

The deaths from cancer during the first ten years were 65,989, and in the second ten years 90,808, giving an increase of 22 per cent. in the death-rate. For males the increase in the rate was 25·5 per cent., and for females 20·4 per cent. The maximum rate of increase occurred with males between the ages of 45 and 55, and amounted to 31·5 per cent., and with females between the ages of 55 and 65, the amount being 23·3 per cent. For every hundred deaths of males there were 230·4 deaths of females in the first ten years, and 220·8 in the second ten years.

Cephalitis.

This disease merits attention as being one which is gradually becoming more fatal to infants and young children. In the first ten years it was the cause of 35,331 deaths, and in the second ten years of 45,827, the increase in the rate of mortality being 15·0 per cent.; but with male infants under one year of age it amounted to 32·5 per cent., and for infants and young children under five years it was 26·1 per cent, while for males at all ages above fifteen there was a decrease of 11·8 per cent.

The gradual and steady change which has taken place during the last twenty-five years in the incidence of mortality from cephalitis upon different ages will be seen from the following numbers:—In the seven years 1848–54 the number of deaths under one year was 14·6 per cent. of the total number of deaths at all ages; in the following seven years it was 16·8 per cent.; in the next six years 18·3 per cent.; and lastly, in the six years 1868–73, it was 20·1 per cent. The percentages of deaths at all ages under five years for these periods were 42·6, 46·7, 49·4, and 52·8.

The total number of deaths in each ten-yearly period

from the remaining non-preventible diseases which have increased in fatality were as follows :—

	10 years, 1854–63.	10 years, 1864–73.
Lung-disease	35,390	49,503
Liver-disease	42,778	57,001
Kidney-disease	21,186	27,910
Tabes mesenterica	50,983	66,789
Rheumatism	19,840	25,415
Brain-disease.....	43,727	54,979
Apoplexy	87,750	110,011
Premature birth	74,670	91,851
Paralysis	91,697	111,994
	468,021	595,453

The average increase from all these diseases is 14·6 per cent., which represents an extra loss of more than 77,000 lives, or more than double the number saved by the improvement in the infectious diseases.

Among the diseases which have diminished in fatality, more than 80 per cent. of the total number of deaths were caused by phthisis, old age, convulsions, and pneumonia. The diminution in the number of deaths from old age will perhaps be regarded as an unfavourable result which is hardly compensated by the diminished fatality among infants and young children from convulsions.

The deaths from old age and convulsions in the ten-yearly periods were :—

	10 years, 1854–63.	10 years, 1864–73.
Old age.....	272,434	282,490
Convulsions.....	251,330	262,170

Phthisis.

The disease which caused the greatest number of deaths in both the ten-yearly periods was phthisis. In the first ten years the number of deaths was 508,210, and in the second 532,780 but allowing for the increase of population

there was a decrease of 7·1 per cent. in the rate of mortality. This decrease, however, was not equally shared by the two sexes, the decline of the rate among males being only 2·4 per cent., while among females it amounted to 12·6 per cent.; nor was there a decrease at all ages, but, on the contrary, between the ages of 25 and 65 there was an increase of 4·5 per cent. More than half the deaths from phthisis occurred at ages under 35 years, and more than 45 per cent. between the ages of 15 and 35.

Pneumonia.

The deaths from pneumonia in the first ten years were 242,757, and in the second 228,069, the decrease in the death-rate being 16·6 per cent. With males the decrease was 15·6 per cent., and with females 18·8 per cent. There was, however, as in the case of phthisis, an increase between the ages of 25 and 65, the amount being 13·7 per cent. In the first ten years the deaths under five years of age were 68·1 per cent. of the deaths at all ages; but in the second ten years the proportion was reduced to 60·8 per cent.

The deaths from dropsy, asthma, enteritis, scrofula, and hydrocephalus in the two ten-yearly periods were as follows:—

	10 years, 1854–63.	10 years, 1864–73.
Dropsy	82,085	63,485
Asthma	42,907	36,173
Enteritis	32,842	29,781
Scrofula	30,219	28,223
Hydrocephalus	73,620	73,652
	261,673	231,314

The mean decrease in the rate of mortality from these

diseases was 21·6 per cent., which represents a saving of 63,886 lives.

There is one disease which, owing to the deaths caused by it not averaging 2500 per annum during the last ten years, has not been included in the list of those which have increased in fatality, but which, I think, ought not to pass without notice. This is nephria or Bright's disease. In the ten years 1854–63 the deaths from it were 11,948; but in the following ten years they were 23,802. The increase in the death-rate from this cause therefore amounted to 76·6 per cent., or more than double the rate of increase of bronchitis.

It is very commonly supposed that all the zymotic diseases are preventible, or under the control of sanitary authorities; and it may be objected that I have taken only the eight infectious diseases to test the utility of the sanitary measures which have been carried out during the last twenty or thirty years. To meet this objection it may be well to state that the total number of deaths from all the zymotic diseases in the ten years 1854–63 was 973,248, and in the following ten years 1,109,824. These numbers give an increase of 1·08 per cent.; and therefore, if sanitary measures are to be tested by the impression they make upon the death-rate from the whole class of zymotic diseases, it is evident that the measures hitherto adopted have entirely failed to produce any improvement of the public health.

The results which I have now laid before the Society, though, perhaps, not so full and complete as might be desired, are, it seems to me, quite sufficient to justify the following conclusions:—

(1) That sanitary measures, tested by their effect on the diseases which are universally admitted to be infectious, have produced a slight improvement in the public health,

which is equivalent to a saving of one life in every 134, or a reduction of 0·17 in the general death-rate; but tested by their effect on the whole class of zymotic diseases, they have caused no improvement whatever, and have not even prevented an increase in the mortality from these diseases, which is equivalent to an extra loss of life in every 417.

(2) Considering the great increase in the fatality of small-pox when that from all other infectious diseases has diminished, that it has now become relatively much more prevalent among adults than at any time since vaccination was introduced, that the experience of the last and of the present epidemic has furnished abundant proof that vaccination does not now, as it did in Jenner's time and for many years after, afford an almost certain protection against its attacks, and that the question of the probab i lity of its gradually becoming greatly modified and producing results that may be seriously unfavourable to the public health has, apparently, never been considered and investigated, it is desirable that an authoritative inquiry into the whole subject should be at at once set on foot, in order to ascertain, if possible, why vaccination has now become so much less efficacious as a preventive of small-pox than it was for many years after its general adoption, what has been the true cause of the great increase of small-pox during the last fifteen to twenty years, and why adults have become so much more liable to its attacks.

(3) That the reduction in the rate of mortality from the class of infectious or preventible diseases has been more than counterbalanced by an increase in the rate from the non-preventible class; and that, owing to the changes which have taken place in the latter, the mortality from diseases which run their course rapidly and cause comparatively sudden deaths, has greatly increased.

X. *On the Directions of the Face-joints of Oblique Arches.* By J. B. Millar, B.E. Communicated by Professor O. Reynolds, M.A.

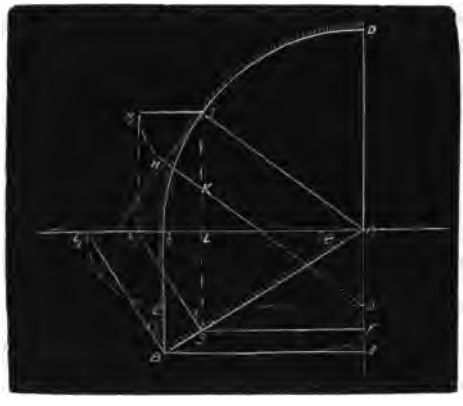
Read October 31st, 1876.

It was observed by Mr. Buck, as described in his well-known 'Essay on Oblique Bridges,' that, in the drawings of the faces of oblique arches, when the face-joints were drawn straight lines and produced, they all met in one point on the vertical line intersecting the axis of the arch. That point he called the "focus," and its distance from the axis of the arch the "excentricity." He gives no reason for this convergence of the face-joints; but taking it as a fact, he determined the focus for the joints at the springing, and so obtained an expression from which the excentricity may be found for any given arch.

In the 'Philosophical Magazine' for January 1862, G. B. Airy, Astronomer Royal, demonstrated, by the method of analysis, the truth of the theorem for the tangents to the face-joints at points equally distant from the axis—such, for instance, as the points on the ellipse formed by the intersection of the soffit with the plane face. The following demonstration, besides being shorter than that given by the Astronomer Royal, depends only on elementary geometrical principles, and, it is believed, will convey a clearer notion of the state of the case, at least to engineers, whose point of view for subjects of this kind is frequently different from that of the mathematician.

The coursing joints of an oblique arch are *helical surfaces* of equal pitch; that is to say, they are surfaces generated

by a straight line which revolves uniformly about the axis, at right angles to it, and which at the same time has a uniform motion along the axis, the ratio of these two motions being constant for all the joints. A face-joint is the line of intersection of one of these helical surfaces with the plane of the face. It will readily be seen that these face-joints are curves; but the curvature is very slight for the ordinary dimensions and angles of arches. The only joint which could be straight would be a vertical one; and there is none such in the face of an arch.



Let BC represent the projection of half the face of the arch on a horizontal plane containing the axis, which passes through C, and bg'D the elevation of the soffit on a plane perpendicular to the axis; it is required to show that the tangents to the face-joints on any points on BGD (G is the point of the soffit of which gg' are the projections) meet at one point on the vertical line CA.

The tangent to the face-joint at any point is the line of intersection of the plane face and the tangent plane to the helical surface at that point.

First.—To find the tangent to the face-joint at the point

11. The tangent plane to the helical surface at B contains the generator AB and the tangent to the helix which passes through B, that helix being the line of intersection of the helical surface with the cylinder which is the soffit of the arch. These two lines determine the tangent plane. As the tangent to the helix at B lies in the tangent plane to the cylinder and makes an angle ϕ (which is the angle of skew-back of the soffit) with Bb, the point in which it intersects the vertical plane of projection, may be found as follows:—

Draw BE_1 making an angle ϕ with Bb, and make bE equal $b E_1$; bE is the vertical trace of the tangent to the helix at B; $b E_1$ is, in fact, the position of the tangent line in question when the tangent plane to the cylinder is turned about Bb till it becomes horizontal. But the tangent plane to the helical surface at B also contains the line AB, which is parallel to the vertical plane of projection; consequently EO, parallel to bC, is the vertical trace of the tangent plane. Hence it follows that the line of intersection of this plane with the plane face Bb passes through O (in elevation); and since it also passes through B, Ob is the vertical projection of the tangent to the face-joint at B.

Second.—Let any other point G be taken, of which the projections are g and g'. As before, the tangent plane to the helical surface is determined by the generator GF and the tangent line to the helix traced on the soffit through G.

The tangent to the helix lies in the plane Gg'H; so that by drawing $g h_1$, making the angle ϕ with gL and setting off g'H equal to Lh_1, the vertical trace of the tangent to the helix in G is found. Hence the vertical trace of the tangent plane to the helical surface at G passes through H and is parallel to g'C, the vertical projection of the generator at G.

Draw H K parallel to Cg', meeting the vertical line g'L at K.

As the triangles K Hg' and C Lg' are similar,

$$\frac{\mathrm{K}g'}{\mathrm{H}g'}=\frac{\mathrm{C}g'}{\mathrm{CL}}=\frac{\mathrm{C}b}{\mathrm{CL}}=\frac{\mathrm{B}b}{g\mathrm{L}}=\frac{b\mathrm{E}_1}{\mathrm{L}h_1}.$$

But $\mathrm{H}g'=\mathrm{L}h_1$ by construction.

Therefore $\mathrm{K}g'=b\mathrm{E}_1=\mathrm{CO}$.

Consequently the line HK produced will pass through O, and the vertical projection of the tangent to the face-joint at G is Og'.

In the same way it may be shown that the tangent to a face-joint at any other point of AGD passes through O. Q.E.D.

Let the angle $\mathrm{BC}b=\vartheta$=angle of obliquity of the arch.

Then $\mathrm{B}b=r\tan\vartheta$, where $r=\mathrm{C}b$, the radius of the soffit.

$\mathrm{E}_1b=\mathrm{B}b\tan\phi=\mathrm{CO}$.

Therefore $\mathrm{CO}=r\tan\vartheta\tan\phi$.

This is the same value for the excentricity as that found by Buck, and is perfectly general, depending, as it does, only on the radius of the cylinder, the angle of skew-back for that cylinder, and the obliquity of the arch. The simplest method, however, of obtaining the point O is that shown above by means of a geometrical construction.

XI. *On a Portrait of Tycho Brahe.*

By Samuel Crompton, M.D.

Read October 31st, 1876.

Dr. Crompton exhibited a portrait in oil of Tycho Brahe, and gave some particulars respecting the known portraits

of Brahe. This picture is on canvas, and is 3 feet and $3\frac{1}{4}$ inches high, and 2 feet and $6\frac{1}{2}$ inches wide. It represents a man of ruddy complexion, standing and looking forwards. He is bareheaded, has little hair, and that short, on the head, of a yellowish colour verging to red. He has very long moustaches, and a short beard. He wears a wide ruff round the neck edged with wide open lace. He wears a black jacket buttoned down to the waist with gold buttons; and he is decorated with the Danish Order of the Elephant and Castle, suspended by a gold chain from his neck, with a portrait in profile of the king in front of the body of the elephant, and below the elephant the letters MHZGA. He has a broad belt of gold round the waist. The right hand is nearly at a right angle with the body and rests on his side; and on its little finger there is a curious twisted ring of five turns. In his left hand he holds a pair of gloves; and on the left forefinger there is a large signet ring. There is also a ring on the fourth and another on the fifth finger. He has wristbands edged with open lace.

In the right upper corner of the picture (that is, to B.'s right) there is a curious emblematic design, consisting of a round tapering column springing from a square base, around which, at its foot, are waves. Over the monument is a canopy suspended by a strong chain, a few links only of which are visible, the top being lost in clouds; and the chain itself has flames playing round it. Two Æolic heads (one on each side) are represented as blowing towards the canopy and column. Lower down, and to the right and left of the column, are two hands (one on each side) holding each a jug from which water flows. Clouds and lightning surround the background, the wrists of the hands holding the jugs, and also the Æolic heads. Round the monument is a label, not entirely decipherable, with the words "Stans (tectus?) in solido;" then follows an indistinct word and

"igne e · tunda" (*sic*); "Igne et unda" was, no doubt, intended. In the left upper corner, in large and distinct letters, is this inscription—"Effigies Tychonis Brahe, Otton. Da. Anno 50 completo quo post diutinum in patria exilium libertati desideratæ divino provisu restitutus est."

Dr. Crompton pointed out, by references to Gassendi's 'Tychonis Brahe Vita,' 4to, Hagæ Comit. 1656, that the inscription referred to Brahe's departure from Denmark, and that the "exilium *in* patria" was an allusion to his residence on his island of Huenna, in his Observatory, away from the court for twenty years. The emblematic picture evidently implies that nothing (not all the elements) could destroy the monument which he had erected to his reputation by his observations, and that they would be protected by Providence.

Dr. Crompton exhibited two engraved portraits of Tycho, the first of them engraved by Meurs, in which he is surrounded by heraldic coats of arms, and with an inscription to the effect that the portrait represented him on the completion of his 40th year in 1586. The second engraving was by Haas, of Copenhagen, and is without a date. In both of these engravings Brahe is represented with a cap on his head and wearing, suspended by a double chain, the Danish Order of the Elephant and Castle, but without the portrait of the king, and without the letters on Dr. C.'s portrait.

Dr. C. proceeded to show from Gassendi, page 71, that King Frederick had given him the Order, and that subsequently his son, King Christian (Gassendi, page 116), had given him a second badge, which he, himself, wore, that the king had taken it off his own neck and put it round Brahe's neck; and he particularly mentions that this second badge had a portrait of the king upon it, as it actually is represented in Dr. Crompton's portrait. Gassendi men-

tions an engraving of Brahe in which he is represented wearing both of the Orders so given to him.

As the inscription on Dr. Crompton's portrait says that it was painted on the completion of his 50th year, it is a representation of Tycho ten years later than the engraving by Meurs of 1586; and the tenor of the emblems and of the inscription seems to be conclusive that the picture was painted after Brahe had left Denmark, which he did (Gassendi, p. 142) about Midsummer, 1597, whence he went to Rostock; but this place he left for fear of the plague, after the end of October, when, by invitation, he went to the Castle of Wandsburg, near Hamburg, to stay with his friend Ranzovius. Immediately on his arrival there he composed a long poem in Latin (given by Gassendi at pages 143, 144, and 145) in which he complains bitterly of his country's blindness and want of appreciation of his notable services, which he lauds in a remarkable way, but not beyond their deserts. In the course of this poem are passages which justify the emblem on Dr. C.'s portrait, and several lines which bear closely on the inscription on his picture.

The following may be quoted (p. 145):—

> "Pro quibus, o Superi, mihi gratia reddita talis,
> Sex ego cum natis, matreque ut exsul agam.
> Sum tamen haud exsul, *libertas contigit ampla;*
> *Exsilium in patria* verius ante tuli.
> Nunc ego prosper ago; proprii nunc juris alumnus;
> Nunc mihi pro patria maximus orbis adest."

From these facts Dr. C. conjectures that the portrait was painted between the end of October and the 13th of December 1597. It could not have been painted later than the 13th of December; for then the inscription would have been "anno 51 completo;" and as this poem is addressed to Ranzovius, the presumption is that it was written after Brahe's arrival at Ranzovius's. And Dr. C. conjectures that the portrait may have been painted to be engraved for

Brahe's 'Mechanica,' which he immediately began to print at Hamburg, whilst he remained at Ranzovius's, and that there may not have been time to have it engraved; or, may be, it was too self-glorious.

The residence of Ranzovius, half a mile from Hamburg, existed till ten or fifteen years ago, when it was pulled down, and a full-length portrait of Brahe disappeared, representing him with an astronomical instrument in one hand.

Dr. C. exhibited a photograph from his picture, taken by an able photographer, Mr. White, Crescent, Salford.

Note.—In the description of Brahe's dress there is a serious omission, viz. that he is represented in a cloak with a high collar. Further particulars respecting this picture will be found in 'Nature,' October 11, 1877.

XII. *On the Types of Compound Statement involving four Classes.* By Professor W. K. Clifford, M.A., F.R.S. Communicated by Professor W. S. Jevons, M.A., F.R.S.

Read January 9th, 1877.

Professor Stanley Jevons has enumerated* the types of compound statement involving three classes, among which the premises of a syllogism appear as a type of fourfold statement. He propounded at the same time the corresponding problem of enumeration for four classes, which is solved in the present communication. The reader is referred to the paper or the book just mentioned for further

* 'Proceedings of the Manchester Philosophical Society' vol. vi. pp. 65–68, and 'Memoirs,' Third Series, vol. v. pp. 119–130. 'The Principles of Science,' vol. i. pp. 154–164.

explanation of the nature and purpose of the problem than is to be found in art. 1. It may, however, be premised that the letters A, B, C, D denote four *classes* or *terms* (for example, hard, wet, black, nice), and that, according to a convenient notation of De Morgan's, the small letters *a*, *b*, *c*, *d* denote the complementary classes or contrary terms (not hard, not wet, not black, not nice). A *simple* statement is of the form ABCD=0 (no hard, wet, black, nice things exist, or, which is the same thing, all hard, wet, black things are nasty). The statement ABC=0 (no hard, wet, black things exist, or all hard, black things are dry) is to be regarded as made of these two, ABCD=0, ABC*d*=0 (no hard, wet, black, nice things exist, and no hard, wet, black, nasty things exist) and so is called a *compound* (in this case a *twofold*) statement. The notion of *types* is defined in art. 1.

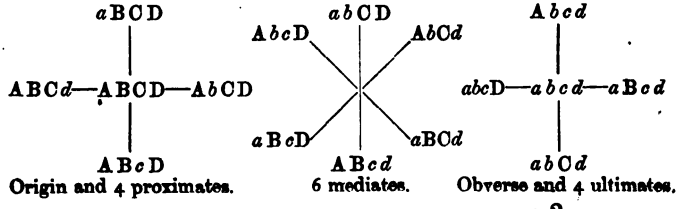
1. Four classes or terms A, B, C, D, give rise to sixteen cross divisions or *marks*, such as A*b*C*d*. A denial of the existence of one of these cross divisions, or of any thing having its mark (such as A*b*C*d*=0), is called a simple statement. A denial of two or more cross divisions is called a compound statement, and, moreover, twofold, threefold, etc., according to the number denied.

When two compound statements can be converted into one another by interchange of the classes A, B, C, D with each other or with their complementary classes *a*, *b*, *c*, *d*, they are called *similar*; and all similar statements are said to belong to the same *type*. The problem before us is to enumerate all the types of compound statement that can be made with four terms.

2. Two statements are called *complementary* when they deny between them all the sixteen marks without both denying any mark, or, which is the same thing, when each denies just those marks which the other permits to exist. It is obvious that when two statements are similar, the

complementary statements will also be similar; and, consequently, for every type of n-fold statement there is a *complementary type* of $\overline{16-n}$-fold statement. It follows that we need only enumerate the types as far as the eighth order; for the types of more than eightfold statement will already have been given as complementary to types of lower orders. Every eightfold statement is complementary to an eightfold statement; but these are not necessarily of the same type.

3. One mark ABCD may be converted into another A*b*C*d* by interchanging one or more of the classes A, B, C, D with its complementary class. The number of such changes is called the *distance* of the two marks. Thus in the example given the distance is 2. In two similar compound statements the distances of the marks denied must be same; but it does not follow that when all the distances are the same the two statements are similar. There is, however, as we shall see, only one example of two dissimilar statements having the same distances. When the distance is 4, the two marks are said to be *obverse* to one another, and the statements denying them are called *obverse* statements—as ABCD, *abcd*, or, again, A*b*C*d*, *a*B*c*D. When any one mark is given (called the *origin*), all the others may be grouped in respect of their relations to it as follows:—Four are at distance *one* from it, and may be called *proximates*; six at distance *two*, and may be called *mediates*; four at distance *three*, and may be called *ultimates*. Finally, the obverse is at distance *four*.



It will be seen from the above table that the four proximates are respectively obverse to the four ultimates, and that the mediates form three pairs of obverses. Every proximate or ultimate is distant 1 and 3 respectively from such a pair of mediates. Thus each proximate or ultimate divides the mediates into two classes; three of them are at distance 1 from it, and three at distance 3. Two mediates which are not obverse are at distance 2. Two proximates or two ultimates, or an ultimate and a proximate which are not obverse, are also at distance 2.

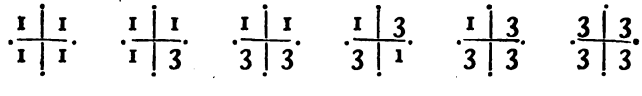
This view of the mutual relations of the marks is the basis of the following enumeration of types.

4. There is clearly only one type of simple statement. But of twofold statements there are four types; viz. the distance may be 1, 2, 3, or 4; and so, in general, with n classes there are n types of twofold statement.

5. A compound statement containing no pair of obverses is called *pure*. In a threefold statement there are three distances; one of these must be not less than either of the others. If this be 2, the remaining mark must be at odd distance from both of these or at even distance from both; thus we get the types 1, 1, 2, and 2, 2, 2. If the not-less distance be 3, the remaining distances must be one even and the other odd; the even distance must be 2, the odd one either 1 or 3; and the types are 1, 2, 3; 2, 3, 3. Thus there are 4 pure threefold types. With a pair of obverses, the remaining mark must be at odd or even distance from them; 1, 3, 4; 2, 2, 4. In all six threefold types observe that there is necessarily one even distance.

6. *A fortiori*, in a fourfold statement there must be one even distance. In a pure fourfold statement this distance is 2. From this pair of marks let both the others be oddly distant; then they must be evenly distant from one another *i. e.* at distance 2, obverses being excluded. The odd

distances are 1 or 3; and it will be easily seen that the following are all the possible cases:—



In these figures the dots indicate the four marks, the cross lines indicate distance 2, and the other figures the distances between the marks on either side of them. Next, from the pairs of marks at distance 2 let one of the others at least be evenly distant, *i. e.* at distance 2. Then we have three marks which are all at distance 2 from one another; and it is easy to show that they are all proximates of a certain other mark. For, select one of them as origin; then the other two are mediates which are not obverse, and which consequently are at distance 1 from some one proximate. With this proximate as origin, therefore, all three are proximates. We have therefore only to inquire what different relations the fourth mark can bear to these three. It may be the origin, its obverse, the remaining proximate, its obverse, or one of two kinds of mediates, viz. at distance 1 or 3 from the remaining proximate. Thus we have 6 types, in which the distances of the fourth mark from the triad are respectively 1 1 1, 3 3 3, 2 2 2, 2 2 2, 1 3 3, 1 1 3. The third and fourth of these are especially interesting, as being distinct types with the same set of distances; I call them *proper* and *improper groups* respectively: viz., a proper group is the four proximates of any origin; an improper group is three proximates with the obverse of the fourth. On the whole we get 12 types of pure fourfold statement

7. In a fourfold statement with *one* pair of obverses, take one of them for origin; the remaining two marks must then be either a pair of proximates or ultimates, a proximate and an ultimate, a pair of mediates, or a proximate or ultimate with one of two kinds of mediate—in all, 5 types,

with the distances $1\,3^2$, $1\,3$; $1\,3^2$, $3\,1$; $2\,2^2$, $2\,2$; $1\,3^1$, $2\,2$; $1\,3^3$, $2\,2$. With *two* pairs of obverses they must be either at odd or even distances from one another; two types. Altogether $12+5+2=19$ fourfold types.

8. In a *pure* fivefold statement there is always a triad of marks at distance 2 from one another. For there is a pair evenly distant; if there is not another mark evenly distant from these, the remaining three are all oddly distant, and therefore evenly distant from one another. First, then, let the remaining two marks be both oddly distant from the triad. In regard to the origin of which these are proximates, the two to be added must be either two mediates, like (of two kinds) or unlike, or a mediate of either kind with the origin or the obverse; 7 types. Next, if one of the two marks be evenly distant from the triad, it must form with the triad either a proper or an improper group of four. To a proper group we may add the origin, the obverse, or a mediate; to an improper group, the origin or the obverse (the mediates give no new type), 5 types; or, in all, 12 pure fivefold types.

9. In a fivefold statement with one pair of obverses there must be another pair of marks at distance 2. We have therefore to add one mark to each of the following three types of fourfold statement,—a pair of obverses together with (1) two proximates, (2) a proximate and an ultimate, (3) two mediates. To the first we may add another proximate, an ultimate or a mediate of three kinds, viz. at distances $1\,1$, $1\,3$, $3\,3$ from the two proximates; 5 types. To the second if we add a proximate or an ultimate, we fall back on one of the previous cases; but there are again three kinds of mediates, at distances $1\,1$, $3\,3$, $1\,3$ from the proximate and ultimate; 3 types. To the third we may add another mediate, whereby the type becomes a proper group together with the obverse of one of its marks, which is the same thing as an improper group together with the

obverse of one of its marks—or a proximate or ultimate which are of three kinds, at distances 1 1, 1 3, 3 3 from the two mediates; 4 types. Thus there are 12 fivefold types with one pair of obverses. With two pairs of obverses at odd distances, there is only one type, all the remaining marks being similarly related to them; at even distance the remaining mark may be evenly or oddly distant from them; 2 types. On the whole we have $12+12+3=27$ types of fivefold statement.

It is to be remarked that there is no pure fivefold statement in which all the distances are even, and that, if there is only one pair of obverses with all the distances even, the type is a proper group together with the obverse of one of its marks.

10. We may now prove, as a consequence of the last remark, that a pure sixfold statement either contains a group of four with a pair oddly distant from it or consists of two triads oddly distant from one another. For there must be a pair at distance 2: if the other four are all oddly distant from these, they form a group; if one is evenly distant and three oddly distant, we have the case of the two triads; if two are evenly distant, we again have a group. We must add, then, first to a proper group, and then to an improper group, a pair oddly distant from it. To a proper group consisting of the proximates to a certain origin we may add the origin or its obverse with a mediate, or two mediates; 3 types. An improper group is symmetrical; that is to say, if we substitute for any one of its marks the obverse of that mark, we shall obtain a proper group. In this way we shall get four origins distant 1 1 1 3 from the group, and four obverses distant 1 3 3 3; if we add to these the obverses of the marks in the group itself, we have described the relation of the twelve remaining marks to the group. To form, therefore, a pure sixfold statement we may add either two origins or two obverses or an origin and an obverse; 3 types.

In the case of the two triads, since they are oddly distant from one another their origins must be oddly distant; that is, they must be distant either 1 or 3. If they are distant 1, neither, both, or one of the origins may appear in the statement; if they are distant 3, neither, both, or one of the obverses: 6 types. Thus we obtain 12 types of purely sixfold statement.

11. If a sixfold statement contains one pair of obverses, the remaining four marks cannot all be evenly distant from this pair. For in that case they would constitute a group; and it is easy to see that the marks evenly distant from a group, whether proper or improper, do not contain a pair of obverses. We have therefore only these four cases to consider:—

(1) The four marks are all oddly distant from the obverses.

(2) One is evenly distant and three oddly distant.

(3) Two are evenly distant and two oddly.

(4) Three are evenly distant and one oddly.

In the first case the four marks form a group. If this is a proper group, the pair of obverses must be either the origin and obverse of the group, or a pair of mediates; 2 types. If the group is improper, the pair must be an origin and an obverse; 1 type. In the second case we have an origin, an obverse, and a mediate, to which we must add 3 marks taken out of the proximates and ultimates. We may add 3 proximates distant respectively 1 1 3 or 1 3 3 from the mediates (2 types),—or 2 proximates distant respectively 1 1, 1 3, 3 3 from the mediate, and with each of these combinations an ultimate distant either 1 or 3 (6 types). To interchange proximates with ultimates clearly makes no difference; so that in reckoning the cases of 1 proximate and 2 ultimates or 3 ultimates, we should find no new types. In the third case we have an origin, an obverse, and two mediates distant 2 from each other; and to these we have to add either two proximates or a proximate and

an ultimate. The two proximates may be distant from the two mediates 1 1, 1 3, or 1 1, 3 3, or 1 3, 1 3, or 1 3, 3 3; 4 types. The proximate or ultimate must not be respectively distant 1 1, 3 3, or 3 3, 1 1; for then they would form a pair of obverses; there remain the cases 1 1 with 1 1 or 1 3, 1 3 with 1 3, and 3 3 with 1 3 or 3 3; 5 types. In the fourth case we have an origin, obverse, and three mediates distant 2 from one another; the remaining mark must be distant either 1 or 3 from these mediates; 2 types. This makes twenty-two types of sixfold statement with one pair of obverses.

12. If a sixfold statement contains two pairs of obverses, these must be either evenly or oddly distant. If they are evenly distant we have an origin, obverse, and two obverse mediates, to which two other marks are to be added. These may be both evenly distant; taking one of them as origin, it is associated with 5 mediates, so that there is 1 type only. Or both oddly distant; here there are two cases, according as the distances are 1 1, 3 3, or 1 3, 1 3. Or one oddly and one evenly distant; the latter is any one of the four remaining mediates, and then the former is distant 1 or 3 from it; 2 types. If the two pairs of obverses be oddly distant they form an aggregate which is related in the same way to all the remaining twelve marks; viz. any one of these being taken as origin, we have a pair of mediates and a proximate with its obverse ultimate. The thing to be considered, therefore, is the distance between the two marks to be added, which may be 1, 2, or 3, and each in two ways; 6 types.

A sixfold statement with three pairs of obverses is one of two types only; viz. these are all evenly distant when they are the mediates to one origin, or two evenly distant and one oddly distant from both of them.

13. A pure sevenfold statement must consist of a group

and a triad; for it must contain a triad, by the same reasoning by which this was proved for a fivefold statement; and then either all the other four marks are oddly distant from this, and so form a group by themselves, or else one of them is evenly distant from the triad and so forms a group with it. If the group is proper, being the proximates to a certain origin, the triad must consist of two mediates and either the origin, the obverse, or another mediate; and in the latter case the three mediates are distant 1 1 1 or 3 3 3 from some proximate; 4 types. If the group is improper, the triad is either all origins or all obverses, or two origins and an obverse, or an origin and two obverses; 4 types. In all, 8 types of pure sevenfold statement.

14. A sevenfold statement with one pair of obverses must consist either of four marks evenly distant from one another and three oddly distant from them; or of five marks evenly distant from one another and two oddly distant from them. In the former case the pair of obverses may be in the four or in the three. If they are in the four, the three form a triad which are proximates to one origin; and then the pair may be the origin and obverse or a pair of mediates. If the pair are origin and obverse, the other two (at distance 2) are mediates, distance 1 1, 1 3 or 3 3 from the proximate which is not in the triad; if the pair are mediates, the two may be the origin or obverse with a mediate distant 1 or 3 from that proximate (4 types), or two mediates distant 1 1, 1 3, 3 3 from it (3 types). If the pair of obverses are in the set of three marks, the four form a group, which may be proper or improper. If proper, the three may be origin and obverse with a mediate, or a pair of mediates with origin, obverse, or another mediate; 4 types. If improper, the three must be two origins and an obverse, or an origin and two obverses; 3 types.

Five marks evenly distant containing only one pair of ob-

verses, must be a proper group with the obverse of one of its marks; see end of art. 9. To these we may add the origin or obverse of the proper group with a mediate distant 1 or 3 from the extra mark, or else two mediates distant 1 1, 1 3, or 3 3 from that mark; 7 types.

15. A sevenfold statement with two pairs of obverses may have six marks evenly distant from one another and one oddly distant from them; in this case the six are an origin and five mediates in two different ways, or say two pairs and a two; the remaining mark may be distant 1 1, 1 3, or 3 3 from the two, which gives 3 types.

Otherwise the sevenfold statement must subdivide (as in the last case) into five and two or into four and three. If it subdivide into five and two, the two may be a pair or not. In the first case we have a proper group and the obverse of one of its marks, together with the origin and obverse of the group or a pair of mediates; 2 types. In the second case we have five mediates of an origin or its obverse, to which we may add two proximates distant 1 1, 1 3 or 3 3 from the odd mediate, or a proximate and an ultimate distant 1 1, 1 3 or 3 3 respectively from the odd mediate; 6 types.

If the sevenfold statement subdivide into four and three, the two pairs may be both in the four, or one in the four and one in the three. In the former case we have a triad, to which may be added the origin and obverse and a pair of mediates or two pairs of mediates; 2 types. In the latter case the four consist of an origin and obverse and two mediates; we must add a pair consisting of a proximate and an ultimate, which may be distant 1 1, 3 3 or 1 3, 1 3 from the two mediates, and then another proximate or ultimate which may be distant 1 1, 1 3, or 3 3 from the two mediates; 6 types.

16. Three pairs of obverses in a sevenfold statement

may be all evenly distant, or two evenly and the other pair oddly distant from each. If they are all evenly distant they are the mediates to a certain origin or its obverse, and the seventh mark may be the origin or a proximate, 2 types. In the other case we have an origin, obverse, and pair of mediates together with a proximate and its obverse ultimate; we may add a proximate or a mediate, 2 types.

17. A pure eightfold statement must consist of two groups, either both proper or both improper, or one of each. Two proper groups may have their origins distant 1 or 3; 2 types. To an improper group we may add a proper group made of one origin and three obverses or of three origins and one obverse, or an improper group made of four origins or four obverses or two origins and two obverses; 5 types. Altogether there are 7 types of pure eightfold statement.

18. An eightfold statement with one pair of obverses must subdivide into four and four, or into five and three. In the former case we have a pair of obverses, viz. an origin and its obverse, and two mediates; to which we must add a group formed out of the proximates and ultimates. This group may be proper, 1 type, or improper, the mediates being in regard to it two origins, two obverses, or an origin and an obverse; 3 types. In the latter case the five marks must be a proper group with the obverse of one mark, to which we must add a triad made out of the origin, obverse, and mediates of the group. This triad may be the origin or obverse together with two mediates distant 1 1, 1 3, 3 3 from the ultimate, 6 types; or else it may be three mediates distant 1 1 1, 1 1 3, 1 3 3, 3 3 3 from the ultimate, 4 types.

19. An eightfold statement with two pairs of obverses must subdivide into four and four, or into five and three, or into six and two. In the first case the two pairs of ob-

verses may be evenly distant, when the remaining marks form a group either proper, with its origin, obverse, and pair of mediates, or two pairs of mediates, or else improper, 3 types; or oddly distant, when the remainder form one of the six pure fourfold statements enumerated art. 6. Two marks distant 2 from each other may be distant 1 1, 3 3 or 1 3, 1 3 from the pair of obverses which are oddly distant from them; thus each of the six fourfold statements gives 3 types of eightfold statement, except the third, which gives 4; in all, 19. In the second case the three may be a triad or may contain a pair of obverses. If it is a triad, the five are mediates to one origin and its obverse, and we may add three proximates distant 1 1 3 or 1 3 3 or two proximates distant 1 1, 1 3, or 3 3 with an ultimate distant respectively 1 1 or 3 3 from the odd mediate; 6 types. If the three contain a pair of obverses, the five make a proper group with obverse of one mark; to this we may add the origin and obverse of the group with mediate distant 1 or 3 from the ultimate, or a pair of obverse mediates with a mediate distant 1 or 3 as before; 4 types. In the third case the six must be an origin and five mediates, and we may add two proximates distant 1 1, 1 3, 3 3 from the odd mediate, or a proximate and an ultimate, or two ultimates, distant as before; 9 types.

20. In an eightfold statement with three pairs of obverses these may be either all evenly distant, or two of them evenly distant and the other oddly distant from both. In the first case they are mediates to a certain origin and its obverse, and we may add the origin with a proximate or ultimate, two proximates, or a proximate and ultimate; 4 types. In the second case take the oddly distant pair for origin and obverse; then these are associated with two proximates and their obverse ultimates, and we may add the two other proximates, a proximate and an ultimate, a proximate and a mediate (distant 1 1, 1 3, 3 1, 3 3 from this proximate and

the remaining one), or two mediates distant 1 1, 3 3 or 1 3, 1 3 from the two proximates; 8 types.

Lastly, in an eightfold statement with four pairs of obverses they may be all evenly distant, or the statement may subdivide into six and two, or into four and four; in the latter case there are 2 types.

21. To obtain the whole number of types, we observe that for every less-than-eightfold type there is a complementary more-than-eightfold type (art. 2); so that we must add the number of eightfold types (78) to twice the number of less-than-eightfold types (159); the result is 396.

TABLE.

Art.			
4.	1-fold		1
	2-fold, distance 1, 2, 3, 4		4
5.	3-fold, pure, distance 1 1 2, 2 2 2, 1 2 3, 2 3 3	4	
	1 pair obv., dist. 1 3 4, 2 2 4	2	
		6	6
6.	4-fold, pure, two and two :—		
	$\frac{1\|1}{1\|1}$. $\frac{1\|1}{1\|3}$. $\frac{1\|1}{3\|3}$. $\frac{1\|3}{3\|1}$. $\frac{1\|3}{3\|3}$. $\frac{3\|3}{3\|3}$.	6	
	three and one	4	
	group, proper or improper	2	
		12	12
7.	„ 1 pair obv.	5	
	2 pair obv., dist. odd or even	2	
		19	19
8.	5-fold, pure, three and two	7	
	four and one	5	
		12	
9.	„ 1 pair obv. + two prox.	5	
	+ prox. + ult.	3	
	+ two med.	4	
		12	
			12
	2 pair obv., odd dist., 1; even, 2	3	
		27	27

TABLE (*continued*).

Art.					
10.	6-fold, pure, three and three		6		
		four and two	6		
			12		
				12	
11.	„	1 pair obv., two and four	3		
		three and three	8		
		four and two	9		
		five and one	2		
			22		
				22	
12.	„	2 pair obv., odd dist., 6; even, 5		11	
		3 pair obv.		2	
				47	47
13.	7-fold, pure; proper group, 4; improper, 4			8	
14.	„	1 pair obv., four and three	10		
		three and four	7		
		five and two	7		
			24		
				24	
15.	„	2 pair obv., six and one	3		
		five and two	8		
		four and three	8		
			19		
				19	
16.	„	3 pair obv.		4	
				55	55
		Total of less-than-eightfold statements			159
		Complementary more-than-eightfold statements			159
17.	8-fold, pure			7	
18.	„	1 pair obv., four and four	4		
		five and three	10		
			14		14
19.	„	2 pair obv., four and four	22		
		five and three	10		
		six and two	9		
			41		41

TABLE.

Art.			
20. 8-fold, 3 pair obv., all evenly dist.	4		
two evenly dist.	8		
	12	12	
4 pair obv.		4	
		78	78
Grand Total			396

XIII. *On the Law of Force when a Thin, Homogeneous, Spherical Shell exerts no Attraction on a Particle within it.* By J. H. POYNTING, B.A., B.Sc.

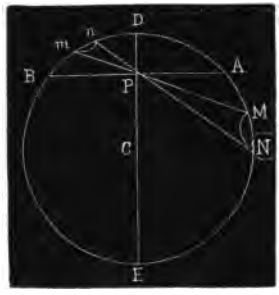
Read March 6th, 1877.

IF a homogeneous, thin, spherical shell of uniform thickness exert no attraction on a particle within it, then the law of the force is the law of nature.

Professor Maxwell uses this proposition ('Electricity,' vol. i. § 74) to deduce the law of the force between electrified bodies, and shows that it proves, far more conclusively than any direct measurements of electrical forces, that the law is that of the inverse square. It would therefore be an advantage to have a simpler proof of such an important proposition than that given by Laplace (Méc. Céleste, liv. ii. ch. 2) and followed by Maxwell. The following seems more simple, as it requires neither integration nor the solution of a functional equation:—

Let P be any point inside the spherical shell, C the

centre of the sphere, D P C E the diameter through P, and A P B perpendicular to C D. In Newton's proof of the



proposition that, if the law of attraction be that of the inverse square, the force at P is zero, the surface is divided into an indefinitely great number of opposite elements by small cones having their vertices at P, and the attractions of each of these pairs of elements are shown to balance each other. We shall first show that if the attraction at P is zero, then it follows inversely that, for at least one position (if not for all positions) of the cone M P *m* besides the position A P B, the attractions of the opposite elements balance each other; and we shall thence prove that the law of attraction must be that of the inverse square.

Let us suppose the cone, with vertex at P, to move round from the position where A B is its axis to any other position M P *m*. At A B the attractions of the opposite sections on P are equal, whatever the law of the force. As the cone leaves A B let us suppose the resultant attraction of the two opposite elements to be no longer zero, but to act, say, towards the centre side of A P B. Then it will either continue towards that side as the cone moves all the way round from A P B to B P A, or it will vanish at some position, and then act in the opposite direction. In the first case we should have a number of forces all acting from P towards

the same side of APB, whose resultant is zero; then each separate force must be zero. In the second case the resultant attraction of the opposite sections vanishes somewhere between AP and EP; then for at least one position of the cone, besides the position APB, the resultant attraction of the opposite sections vanishes.

Since this is true for any position of P, we can show that the law of the force must be that of the inverse square.

In the position where the two opposite sections exert equal attractions, two sections of the same thickness perpendicular to the axis of the cone would also exert equal attractions; for they would bear to each other the same ratio as the two oblique sections made by the sphere, since these two oblique sections make equal angles with the axis of the cone. Then what we have proved is, that for every position of P there are two different distances for which the attractions of the sections of a small cone of equal thickness on a point at its vertex are equal.

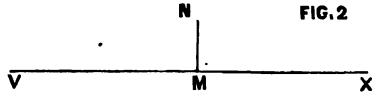


FIG. 2

Let us take VMX to represent the axis of a cone of very small angle of which V is the vertex.

At any point M draw an ordinate MN to represent the attraction of a section of the cone at M of small given thickness on a point at the vertex. Then N will trace out a curve as M moves along VX.

Now take a spherical shell of thickness equal to the thickness of the sections of the cone, and of radius nearly equal to VM, where M is any arbitrary point in VX. Take a point near the centre of this sphere. As a cone moves round with this as vertex, its sections by the sphere must

be always at distances very nearly equal to the radius from the vertex; and, by what we have proved above, for some position of the cone the attractions of the opposite sections must be equal. Therefore (in fig. 2), for two distances very nearly equal to VM the ordinates must be equal to one another. Then the tangent to the curve near N must be parallel to VX. But M is arbitrary; for we can take the sphere of any size. Therefore at all points the tangent to the curve is parallel to VX; and therefore the curve must be a straight line parallel to VX; or, the attractions by sections of the cone of equal thickness are constant, wherever the sections be taken. But the sections are proportional to the direct square of the distance; and therefore the law of the attraction must be that of the inverse square of the distance.

XIV. *On Compound Combinations.*

By Prof. CAYLEY, F.R.S. &c.

Read February 6th, 1877.

PROF. Clifford's paper, "On the Types of Compound Statement involving Four Classes," relates mathematically to a question of compound combinations; and it is worth while to consider its connexion with another question of compound combinations, the application of which is a very different one.

Starting with four symbols, A, B, C, D, we have sixteen

combinations of the *five types* 1, A, AB, ABC, ABCD ($1+4+6+4+1=16$ as before). But in Prof. Clifford's question 1 means $A'B'C'D'$, A means $AB'C'D'$, &c.; viz. each of the symbols means an aggregate of four assertions; and the 16 symbols are thus *all of the same type.* Considering them in this point of view, the question is as to the number of types of the binary, ternary, &c. combinations of the sixteen combinations; for, according as these are combined,

$$\text{No. of types} = \frac{1,2,3,4,\ 5,\ 6,\ 7,\ 8,\ 9,\ 10,11,12,13,14,15}{1,4,6,19,27,47,55,78,55,47,27,19,\ 6,\ 4,\ 1}$$

together.

In the first mentioned point of view the like question arises, in regard to the sets belonging to the five different types separately or in combination with each other; for instance, taking only the six symbols of the type AB, these may be taken 1, 2, 3, 4, or 5 together, and we have in these cases respectively

$$\text{No. of types} = \frac{1,\ 2,\ 3,\ 4,\ 5}{1,\ 2,\ 2,\ 2,\ 1},$$

as is very easily verified; but if the number of letters A, B... be greater (say this $=8$), or, instead of letters, writing the numbers 1, 2, 3, 4, 5, 6, 7, 8, then the question is that of the number of types of combination of the 28 duads 12, 13, ...78, taken 1, 2, 3,...27 together, a question presenting itself in geometry in regard to the bitangents of a quartic curve (see 'Salmon's Higher Plane Curves,' Ed. 2 (1873), pp. 222 *et seq.*): the numbers, so far as they have been obtained, are

$$\text{No. of types} = \frac{1,\ 2,\ 3,\ 4\ \ldots\ldots 24,\ 25,\ 26,\ 27}{1,\ 2,\ 5,\ 11\ \ldots\ldots 11,\ 5,\ 2,\ 1}$$

It might be interesting to complete the series, and, more generally, to determine the number of the types of combination of the $\frac{1}{2}n(n-1)$ duads of n letters.

XV. *On Ternary Differential Equations.*

By Robert Rawson, Esq., Hon. Member of the Society.

Read February 6th, 1877.

1. The following observations have been suggested by reading the interesting communication to the ordinary meeting of November 28th, 1876, "On Ternary Differential Equations," by Sir James Cockle.

With the view of comprehending fully the nature and importance of the step advanced by Sir James Cockle in the communication above referred to, it will be necessary to narrate briefly the exact state of our present knowledge of this subject, as given by Boole and other eminent writers on differential equations.

The substance of this knowledge may be given in two propositions, as follows:—

First. That the differential equation

$$P dx + Q dy + R dz = 0 \quad . \quad . \quad . \quad . \quad . \quad (1)$$

(where P, Q, R are given functions of x, y, z) has a single solution (or, in other words, there is a single relation between the variables x, y, z which will satisfy it), provided the following conditional equation obtains:—

$$P\left(\frac{dQ}{dz}-\frac{dR}{dy}\right)+Q\left(\frac{dR}{dx}-\frac{dP}{dz}\right)+R\left(\frac{dP}{dy}-\frac{dQ}{dx}\right)=\square=0. \quad (2)$$

$\square$ is called by Sir James Cockle the discriminoid.

Second. If the conditional equation (2) does not obtain by virtue of the given functions P, Q, R, then the equation (1) has a *dual solution*—or, in other words, there are two relations between the variables x, y, z which will satisfy it.

2. The first of these propositions was known to Newton and his contemporaries, who, however, regarded all those differential equations which failed to satisfy (2) as absurd and meaningless. Monge supplied the *dual solution*, and thereby removed the reproach of absurdity and meaningless from those differential equations which failed to satisfy the conditional equation (2).

3. Both of the above propositions are considerably circumscribed by a *third proposition* discovered by Sir James Cockle, viz.:—Equation (1) may fail to satify (2) and still be satisfied by a single relation between the variables of which it is composed.

The importance of this proposition can hardly be exaggerated, especially in reference to the doctrine of curved surfaces, &c.; still it might be hazardous, in the present active state of mathematical investigation both here and on the continent of Europe, to say it is new. It is not, however, referred to by Boole, De Morgan, or M. l'Abbé Moigno, the ablest writers on differential equations.

4. Sir James Cockle has shown also that, when equation (1) fails to satisfy the conditional equation (2) and still possesses a single solution, this solution is gathered from the equation $\square = 0$.

The solution here alluded to is not inaptly called the *discriminoidal solution* by Sir James Cockle.

5. The following view of the subject is intended to further elucidate the proposition in art. 4, and also to show a reason for the existence of these *discriminoidal solutions*.

By a reference to the general primitive and its derived differential equation, it will not be difficult to perceive the manner in which the harmony of the conditional equation (2) is disturbed by the legitimate process of elimination between the primitive and its differential equation. Take, for example, a simple case:—

Let $$(y+x^2)z-y=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$
be the primitive. Differentiate in the ordinary way; then

$$(z-1)\frac{dy}{dx}+2xz+(y+x^2)\frac{dz}{dx}=0. \quad . \quad . \quad (4)$$

I prefer writing differential coefficients to differentials. Now equation (4) has evidently a single solution (3), and satisfies the conditional equation (2).

This is all right enough; but now observe the effect produced on the discriminoid $\square$ by eliminating $2xz$ from equation (4) by means of its value as given by equation (3).

From (3) $$2xz=\frac{2xy}{y+x^2}.$$

Substitute this value in equation (4) and it becomes

$$(z-1)\frac{dy}{dx}+\frac{2xy}{y+x^2}+(y+x^2)\frac{dz}{dx}=0. \quad . \quad . \quad (5)$$

Equation (5) has still the single solution (3); but it now fails entirely to satisfy the conditional equation (2).

The discriminoid $$\square=\frac{2x}{y+x^2}\left\{(y+x^2)z-y\right\}. \quad . \quad (6)$$

The discriminoidal solution of equation (5) is therefore a factor of the discriminoid $\square$, in accordance with the proposition of Sir James Cockle, as stated in art. 4.

Let us take another very general equation, viz.

$$\left(\frac{dw}{dy}z-\frac{dv}{dy}\right)\frac{dy}{dx}+\frac{v}{w}\frac{dw}{dx}-\frac{dv}{dx}+w\frac{dz}{dx}=0, \quad . \quad . \quad (7)$$

where w, v are functions of x, y only.

This equation does not satisfy the conditional equation (2), as the discriminoid $\square$ is readily found to be as follows:

$$\square=(wz-v)\left\{\frac{1}{w}\frac{dw}{dx}\frac{dw}{dy}-\frac{d^2w}{dxdy}\right\}. \quad . \quad . \quad . \quad (8)$$

This equation vanishes only when one of the two factors of which it is composed vanishes.

The first factor, viz. $wz-v=0$, satisfies equation (7); therefore it is the discriminoidal solution of (7).

In consequence of $wz-v=0$ being a very general primitive, it is inferred that equation (7) is a very general differential equation which admits of discriminoidal solution.

6. Boole, in page 285, has, by a reference to one of the dual equations, unnecessarily limited the values of the constants in the equation

$$y\frac{dy}{dx}+x+c\sqrt{1-\frac{x^2}{a^2}-\frac{y}{b^2}}\,\frac{dz}{dx}=0,$$

so as to admit of a single solution.

This equation is satisfied by the single solution

$$c^2(z-c)+a^2(x^2+y^2)=a^4.$$

When $a=b$, the quantity C is introduced by integration.

7. The elimination of an arbitrary constant between the primitive and its derived differential equation does not disturb the harmony of the conditional equation $\square=0$.

Let $$(C-R_1)(C-R_2)\ \&c.=0 \quad . \quad . \quad . \quad . \quad (9)$$

be a primitive where C is an arbitrary constant and R_1, R_2, &c. are given functions of x, y, z.

Differentiate (9); then

$$\left\{\frac{dR_1}{dy}(R_2-C)-\frac{dR_2}{dy}(R_1-C)\right\}\frac{dy}{dx}$$
$$+\left\{\frac{dR_1}{dx}(R_2-C)+\frac{dR_2}{dx}(R_1-C)\right\}$$
$$+\left\{\frac{dR_1}{dz}(R_2-C)+\frac{dR_2}{dz}(R_1-C)\right\}\frac{dz}{dx}=0. \quad . \quad . \quad (10)$$

This equation evidently satisfies the conditional equation (2).

Eliminate C from (10) by means of equation (9), and there results

$$(R_1-R_2)^2\ \&c.\ \left\{\frac{dR_1}{dy}\frac{dy}{dx}+\frac{dR_1}{dx}+\frac{dR_1}{dz}\frac{dz}{dx}\right\}$$
$$\left\{\frac{dR_2}{dy}\frac{dy}{dx}+\frac{dR_2}{dx}+\frac{dR_2}{dz}\frac{dz}{dx}\right\}\ \&c.=0. \quad (11)$$

Now, each of the factors equated to zero evidently satisfies the conditional equation (2).

8. It is readily seen that (11) is satisfied by (9) the general primitive, and also by $R_1 - R_2 = 0$, an equation which is not included in the general primitive by giving to C any value independent of x, y, z. Therefore $R_1 - R_2 = 0$ is a singular solution of (11).

The following simple rule derived from the above is of some importance in finding the singular solution of a differential equation from its general primitive.

Solve the general primitive algebraically with respect to the arbitrary constant, the roots being generally functions of x, y, z. *The condition of equal roots is a singular solution.*

Professor Cayley informs me that the above rule is well known at Cambridge. This may be so; still I have never seen it in print, and it is not mentioned by Boole in his 'Differential Equations.' It was discovered by me some fifteen years ago, when I communicated it to my pupils and mathematical friends, amongst whom I may mention the names of Rev. Robert Harley, F.R.S. &c. and the late Mr. Parkis, senior Wrangler of 1864, and Vice-Principal of the School of Naval Architecture at Kensington. It may be further stated that if a differential equation of two variables be solved algebraically with respect to $p \left(= \frac{dy}{dx}\right)$, the roots being functions of x, y, the condition of equal roots which satisfies the differential equation is a singular solution.

XVI.—*Results of the Monthly Observations of the Magnetic Dip, Horizontal Force, and Declination, made at the Magnetic Observatory of the Owens College, from January* 1874 *to December* 1876, *inclusive.* By Professor THOMAS H. CORE, M.A. Communicated by Professor BALFOUR STEWART, LL.D, F.R.S.

Read January 23rd, 1877.

THIS paper, containing the results of three years' magnetic observations, is in continuation of a paper by Professor B. Stewart, in No. 10, vol. xiii. of the Society's 'Proceedings,' p. 111, containing the results for 1873 of the Observations for Dip and Horizontal Force. For convenience of reference, Dr. Stewart's numbers are incorporated in Tables I. and II.

The observations were made by Mr. Kingdon and myself and latterly by Mr. Poynting; and the two days of the month on which they were made were always as near the end of the third week or beginning of the fourth week as possible. The instruments and methods of observation were the same as before. For the declination, which was not before observed, in addition to the unifilar by Elliott Brothers, a transit-theodolite by Troughton & Simms was used to determine the azimuth of a fixed southern mark—the highest point of the steeple of Heaton-Mersey church. A mean of several transit-observations of the sun gave the south azimuth of the steeple $14^\circ\ 0'\ 25''$.

TABLE I.—*Magnetic Dip.*

	1873.	1874.	1875.	1876.	Mean of 4 years.
	° ′	° ′	° ′	° ′	° ′
January	69 19·5	69 19·5	69 13·8	69 10·7	69 15·62
February ...	14·6	14·7	12·3	13·0	13·65
March.....	17·6	12·4	13·2	12·7*	13·97
April	18·9	16·8	12·4	14·8*	15·72
May	17·7	17·1	13·6	12·5*	15·22
June	16·1	15·3	15·4	16·0	15·70
Mean	69 17·40	69 15·80	69 13·45	69 13·28	69 14·98

	1873.	1874.	1875.	1876.	Mean of 4 years.
	° ′	° ′	° ′	° ′	° ′
July	69 14·6	69 14·8	69 10·8	69 9·9	69 12·52
August	20·2	14·4	13·2	12·7	15·12
September ...	23·8	12·8*	14·4	12·0	15·75
October	16·8	14·4	15·3	13·3	14·95
November ...	14·4	14·5*	14·1	8·2	12·80
December ...	18·3	13·6*	15·0	11·6	14·62
Mean	69 18·02	69 14·08	69 13·80	69 11·28	69 14·30
Yearly means.	69 17·71	69 14·94	69 13·62	69 12·28	69 14·64

The six numbers marked with an asterisk result from observations of one needle only; but as the mean annual difference of the readings for the two needles for these years differed by less than the tenth of a minute, no correction has been applied to them.

The annual and semiannual inequalities of the dip cannot be accurately deduced from four years' observations; but an approximation to the former is exhibited in the following Table.

		Annual decrease.
	° ′	′
Mean dip in 1873.....	69 17·71	
„ „ 1874.....	69 14·94	2·77
„ „ 1875.....	69 13·62	1·32
„ „ 1876.....	69 12·28	1·34
Mean	69 14·64	1·81

The mean annual decrease in the dip, of 1·81 minute, corresponds very well with that of 2′·02 as found from the Kew observations from 1857 to 1874 inclusive, and 1′·90 from the Stonyhurst observations from 1871 to 1874 inclusive.

TABLE II.—*Horizontal Force.*

	1873.	1874.	1875.	1876.	Mean of 4 years.
January	.....	3·694	3·699	3·700	3·6977
February ...	.....	3·691	3·693	3·704	3·6960
March.....	3·693	3·692	3·709	3·695	3·6972
April	.....	3·691	3·703	3·702	3·6987
May	3·686	3·705	3·693	3·701	3·6962
June	3·684	3·700	3·697	3·708	3·6972
Mean	3.6877	3·6955	3·6990	3·7017	3·6971

	1873.	1874.	1875.	1876.	Mean of 4 years.
July	3.704	3·696	3·693	3·699	3·6980
August	3.704	3·709	3·696	3·699	3·7020
September ...	3.715	3·691	3·697	3·705	3·7020
October	3.661	3·691	3·696	3·701	3·6872
November ...	.....	3·689	3·697	3·703	3·6963
December ...	3·686	3.691	3·707	3·702	3·6940
Mean	3·6940	3·6945	3·6977	3·7015	3·6966
Yearly means	3·6916	3·6950	3.6983	3·7016	3·6969

The values of the horizontal force corresponding to the middle of each of the four years, and the annual increase for each year, are as follows :—

	Mean horizontal force.	Annual increase.
Mean horizontal force for 1873	3·6916	
" " " 1774	3·6950	·0034
" " " 1875	3·6983	·0033
" " " 1876	3·7016	·0033
Means	3·6969	·0033

If we multiply the mean horizontal force for each year successively by the secant and the tangent of the mean dip for the same year, we get the following values of the total force and the vertical force for the four years :—

	Mean total force.	Annual diminution.	Mean vertical force.	Annual diminution.
1873	10·4414		9·7670	
1874	10·4288	·0126	9·7523	·0147
1875	10·4275	·0013	9·7498	·0025
1876	10·4264	·0011	9·7469	·0029
Means	10·4360	·0050	9·7540	·0067

TABLE III.—*Declination.*

	1874.			1875.			1876.			Mean.		
	°	′	″	°	′	″	°	′	″	°	′	″
January	.....			.....			20	47	20	20	47	20
February	.....			.....			20	43	30	20	43	30
March	20	51	10	20	49	0	20	35	20	20	45	10
April	20	48	40	20	46	0	20	32	40	20	42	27
May	20	24	30	20	41	50	20	36	0	20	34	7
June	20	35	30	20	40	40	20	33	0	20	36	23
Mean	20	39	58	20	44	23	20	37	58	20	40	22

TABLE III.—*Declination* (continued).

	1874.			1875.			1876.			Mean.		
	°	′	″	°	″	′	°	′	″	°	′	″
July	20	38	20	20	36	0	20	33	50	20	36	3
August	20	31	0	20	29	10	20	36	40	20	32	17
September	20	48	40	20	32	30	20	33	30	20	38	13
October	20	52	40	20	36	40	20	27	40	20	39	0
November	20	43	50	20	43	50	20	41	0	20	42	53
December	20	58	30	20	39	0	20	42	20	20	46	37
Mean	20	45	30	20	36	12	20	35	50	20	39	10
Annual means..	20	43	17	20	39	28	20	36	54	20	39	42

The values of the declination in the above table are not to be depended on to within two or possibly three minutes, as (1) the observation for the torsion of the suspending thread was not always made; (2), the hour of observation not being always the same, the results are affected by the diurnal variation; and (3) the results must also be affected by irregular disturbances.

XVII. *Inquiries concerning a Change in the Position of the Earth's Axis.* By ARTHUR W. WATERS, F.G.S.

Read March 6th, 1877.

I HAD intended at a previous Meeting of the Society to ask certain questions about a possible change of the position of the earth's axis; but as there was more interesting matter brought forward, I postponed it, and have thought it best

to put into a definite shape the questions I wish to bring before the notice of any astronomers or physicists who may feel inclined to examine them.

The cause of the greater warmth in high northern latitudes during the Tertiary period has not, according to the opinion of many, yet received any satisfactory solution; and there seems to be a growing feeling that even if the explanation given by the astronomers of the cause of the cold should be adequate, the explanation of the warmth in previous periods in the arctic regions is much more complicated.

The flora of the Tertiaries and the indication that they give of the climate of the time have been elaborately worked out by Prof. Heer*, who believes that a gradual diminution of temperature can be traced from the earlier Tertiaries to the later, the Eocene in Central Europe showing a temperature 13°–14° Cent. (23°–25° F.) warmer than at present, while the Upper Miocene of Oeningen shows only about 7° Cent. (12° F.) warmer. Prof. Heer also thinks that the flora proves that the climate then, as at present, was colder the nearer we approach the pole.

The flora is the most satisfactory means by which we can draw important conclusions concerning the former temperatures; but these are all borne out by the faunas. At Oeningen, in Switzerland, a fossil herbarium of wonderful richness, preserved in hard marl, allows us to learn much about the conditions which obtained at the time. The number of species of plants from the quarry is 920, yielding a greater number of species of trees than are now found living in Switzerland. The herbaceous plants are less readily preserved than the leaves of trees; but as also 844 species of insects have been found fossil, some of which are known

* 'Flora fossilis Arctica,' 1868–1877, Zürich; 'Untersuchungen ü. d. Klima und Vegetationsverhält. d. Tertiärlandes,' Winterthur, 1860; 'Die Urwelt der Schweiz,' 1865.

only to feed upon genera of herbaceous plants of which no impressions have yet been discovered, the imperfection of the record of this period is shown. This flora has its present parallel in Japan, the Southern United States, and Sicily.

Besides the general comparison of the vegetation, we can arrive at the same conclusion with regard to the greater warmth by other means:—in the first place, by the greater number of species; then by the larger proportion of trees, by the larger number of evergreen species, and, lastly, by the relative time of appearance of the foliage and blossoms; for in the warmer regions there is less interval than in colder countries. The fossil floras, as also the faunas, in England, Italy, Styria, Iceland, Greenland, and Spitzbergen all confirm the conclusions drawn in Switzerland.

We could scarcely expect to have so complete a record from the Arctic regions; but what we have still bears out the same conclusion. The known living Arctic flora a few years ago only included 762 species; but in these regions* 356 Miocene species have already been determined from latitude 70°–77° in Spitzbergen and Greenland, and include *Taxodium distichum* (swamp-cypress of Texas), *Sequoia* allied to *S. Wellingtonia*, Large-leaved Birch, Lime, Oak, Beech, Plane, and even *Magnolia*, at Cape Lyell, Spitzbergen; so that Prof. Heer, by comparison of the localities of these trees says that Waisal, opposite Disco, lat. 70° N., Greenland, had a temperature 30° F. warmer than at present, and the nearest living representatives are found 10°–20° further south. The last Arctic Expedition found Miocene coal at 81°; but, as far as I am aware, no detailed description of the flora has been published. Fossil floras of the Cretaceous, Jurassic, and Carboniferous periods have also been disco-

* 'Flora fossilis Arctica,' vol. iv. (Zürich, 1877).

vered within the Arctic Circle, very much resembling the floras of the same time from the temperate regions of Europe; and these also indicate a much greater warmth.

Many of these plants are unable to resist severe cold, besides requiring a warm summer; and it seems difficult to accept the fact of these flowering and ripening their seeds where the winters are so long and the summers are so short, and, apart from the lower temperature, where the amount of light is so much diminished.

Having put forward the grounds upon which a more satisfactory explanation is desired of the warm periods preceding the glacial condition, we must now examine the position in which geologists are placed by the physicists.

When the vast extension of the glaciers was appreciated, various theories were started to explain the phenomena. Some thought variation of interstellar space was the cause, others the obliquity of the ecliptic, others that different geographical distribution of land and sea would explain it; and this has had great influence, but does not give a sufficient explanation: and the increase in the eccentricity of the earth's orbit was made the explanation; but this was refuted, as the heat received by proximity to the sun in perihelion is balanced by quicker motion. The explanation which is now generally accepted is that of Mr. Croll*, that the glacial periods were brought about *indirectly* from an increase in the eccentricity of the earth's orbit, modified by the obliquity of the ecliptic. In the longer and colder winter more snow fell, which the summer could not melt away; so that the snow-covered earth got little of the warmth of the sun, as so much was required to melt the snow.

It will be seen that Mr. Croll's theory requires constantly

* Various articles in the 'Philosophical Magazine;' and 'Climate and Time,' 1875.

recurring glacial periods. A few have been discovered to order; but it may safely be predicted that some, at any rate, will not stand examination. For, instead of there being any indication of a gradual decrease of temperature as the palæontological record most distinctly shows for the recent glacial period, we are, for the earlier ones, required to believe that where we have found a coral atoll growing, indicating a tropical temperature, a glacial period of greater intensity than that of the ice-age was brought about, and the glaciers transported their débris where the corals just grew. These glaciers were soon tired of their anomalous position and retired, when immediately there was a fauna indicating a temperature warmer than at present, without any palæontological record being left to indicate there had ever been a colder period; so that the phenomena are explained by getting from great difficulties into much greater ones.

Mr. Croll's theories may account for somewhat warmer periods in the Arctic region; but the question remains so unsatisfactory that, while accepting the explanation as an influencing cause, there are many who would like a further one. Sir Henry James*, and Sir John Lubbock†, and Mr. John Evans‡ have suggested a change in the position of the axis, upon different grounds; but the astronomers have said that no change in the geographical position of the axis can have taken place. And this, as far as I can understand it, has rested upon the figure of rotation of the earth, as the equatorial bulge of 26 miles is just the amount which calculation, as worked out first by Newton, showed that the earth ought to have, and which the astronomers said that the earth must have taken before it solidified to its present condition, the argument being based upon the earth being now so rigid that it could not take this form of a semifluid

* 'Athenæum,' 1860, Aug. 25. † Quart. Journ. Geol. Soc. 1846.
‡ Proc. Roy. Soc. 1866.

globe. For, as the earth is constantly going slower in consequence of tidal retardation, it would* 1000 million years ago have rotated one seventh faster; so that the equatorial bulge would have been proportionately greater, if it then existed. But the conclusion drawn has been based upon a preternaturul rigidity of the earth, which is not now maintained by all physicists.

Most stress has been laid on the possibility of a change by continental or local upheavals altering the centre of gravity and thus changing the position of the axis; but beside this there are two which can effect a change—the unequal removal of water by elevation of land displacing the ocean, and, second, the removal of matter in solution; and these two I will consider, commencing with the last, which works so invisibly.

Taking, according to Mr. Mellard Reade†, about 100 tons of solid matter removed in solution by the drainage from

* 'Geol. Dynamics,' by Sir W. Thomson.

† "On Geological Time," Pres. Address to the Liverpool Geol. Soc., 1876–77, by T. Mellard Reade, C.E., F.G.S. These figures fell into my hands after I had commenced to prepare this paper, and have been most useful; and I gladly acknowledge the great assistance they have been to me. The calculations are based upon analyses given in the Sixth Report of the Rivers-Pollution Commission for England, and such data as can be obtained from other rivers which have been examined; and this serves as a basis for calculation of other areas where the amount brought down has not been so carefully examined.

Mr. Reade's paper is written to show that the rate of denudation indicates that the time the sedimentary rocks have been forming is much greater than is allowed by the physicists; and according to his calculation 10 miles of sedimentary strata, if formed at the present rate of denudation, would require 526 million years; and he also points out how long it would require to derive the salts in solution in the ocean at the present rate, and says that it would require 200 million years to renew the chlorides. This is a most useful paper, as showing how great a length of time is required, and certainly does carry the organic life on the globe very far back; but I think his figures will require modification by such considerations as the amount dissolved by the ocean from the detritus and erosion of the coast and subaqueous eruptions, the former greater rainfall, higher tides, and the warmer water holding more salts in solution.

each square mile, we get about 5000 million tons removed from the land-surface each year, so that in ten years a weight equal to that of Vesuvius* is removed from the land to the oceanic area by this means.

If an equal amount were removed from each latitude, and the deposition were also regular, it could never have any effect; but the deposition takes place most irregularly, since, from the northern hemisphere, with about 37,000,000 square miles, 3700 million tons will be removed per annum, while from the southern hemisphere only about 1300 million tons; and, dividing the amount to be deposited proportionally in the water-area, the north gets 2073 million tons, and the south 2935 million tons. Now if we suppose there is no subsidence, and allowing for the difference between sp. gr. of solid matter in solution and water, we still get a difference of 863 million tons; and this added to the 2400 million tons surplus removed from the north gives a gain for the southern hemisphere of 3230 million tons over the northern, or one Vesuvius in fifteen years.

But it is not at all the case that the matter will be deposited equally over the water-area; for it will be abstracted much more rapidly in the warmer zones, especially the tropical, as it is the warm waters that teem the most with animal life. Here it is that the coral grows, and here and in the temperate region that the *Lithothamnium* and other calcareous sea-weeds of the group of the Melobesiacea grow and have formed such massive deposits in the Tertiary period; and the Bryozoa and Testacea also here occur in greatest abundance.

If we look at the map of the world or a globe, we shall find that the area, as bearing on this subject, from which most weight will be removed will be that of the Asiatic continent; and on the other hand the locality where the

* Mallet, "On Volcanic Energy," Trans. Royal Soc.

weight will be most increased is the Pacific Ocean; that is to say, the area of greatest removal is antipodal to the area of increase, which is in the warm region, where we know the growth is the most rapid—in fact, is just the coral-area, where the researches of Darwin and Dana have acquainted every one with the steady growth of the coral reefs in the area, which is, or was recently, subsiding. The special conditions of this area are rendering it the one in which the greatest weight is the most readily added. If we divide the earth into a land- and water-hemisphere, with England as a centre, the gain of the weight of the water-hemisphere is about 4300 million tons.

It must be self-evident to any one that a sufficient weight added (locally) between the equator and the poles can alter the position of the earth. This is as evident as that a weight will draw down the pan of a balance; and the question remains, How much is sufficient to do this in an appreciable way?

Newton, in his 'Principia'*, says:—Let there be added anywhere between the pole and the equator a heap of new matter like a mountain, and this, by its perpetual endeavour to recede from the centre of its motion, will disturb the motion of the globe, and cause its poles to wander about its superficies, describing circles about themselves and their opposite points.

The amount of change that can be effected depends upon the proportion of added matter (localized to one spot for calculation) to the mass of the equatorial bulge; and this we find clearly put forward in a paper by Sir G. B. Airy† to refute the theory that a change could have been caused by the elevation of mountain-masses, showing how great they must have been, as a weight one thousandth part of

* Newton's 'Principia,' translated by Motte, 1848, New York.

† 'Athenæum,' Sept. 22, 1860.

the equatorial bulge only moves the axis two or three miles.

This statement of Sir G. B. Airy has been used to see what effect special alterations could have given, and by means of simple arithmetic. The results thus obtained are roughly identical with the figures which have been worked out exactly by Sir William Thomson* and by Mr. G. H. Darwin, M.A.†, in a paper read before the Royal Society, of which only a tantalizing abstract has yet been published, thus corroborating Sir G. B. Airy's figures.

Mr. G. H. Darwin concludes that a single large geological change, such as those which obtain on the earth, is competent to produce an alteration in the position of the pole of from one to three degrees of latitude. This is principally, I presume, calculated from the effect of distortion on the position on the earth's centre of gravity‡.

* *Loc. cit.*

† Proc. Royal Soc. No. 175, and 'Nature,' Feb. 22nd, 1877.

‡ Since this paper was written 'Le déplacement Polaire' of Dr. Jules Carret has come into my hands. This little work is written to demonstrate from various grounds that the position of the axis has changed, but without inquiring into the cause. The greater part of the book is devoted to proving that the present distribution of land and sea can only be explained by such a change; and this, he thinks, explains the polar land-area, and the antipodal position of nearly all the land- to water-areas; and he shows that the effect of a change of position of the axis, with the unequal diameters of the earth, which are unequally divided by the centre of gravity, will be to cause the land to be antipodal to the water; and the slight exceptions are near to the circumference of an equator dividing the globe into land- and water-hemispheres, which is where the exceptions would be expected.

By these exceptions he concludes that the poles have moved in a curved direction. If we divide the earth by a plane (grand cercle polaire) perpendicular to the equator and to the direction in which the position of the poles is changing, the points of intersection at the equator form two pivots for this motion; and here the effect of readjustment will be a minimum, while before and behind will be an area of elevation and submergence respectively.

If the motion is curved, the plane (g. c. p.) we have just supposed must cut the tangent of this curve at right angles. The changing position of the tangent changes the position of the plane and of the points of intersection, so that points near the equatorial intersection are removed from the ele-

Taking the weight given by Airy to shift the axis two or three miles, we find it is about half the weight of Europe, if we take its average height as 1343 feet.

Now, supposing we could remove a weight equal to that of Asia, 1000 feet high, from the centre of a land-hemisphere about 45° N., and add a similar weight at the antipodes in a sinking Pacific Ocean, leaving the remaining portions in each hemisphere balanced by the natural configuration, we should then have an alteration of from 18 to 27 miles. It is true that at the rate of denudation by solution that has been used for calculation, it would take to remove so great a thickness as that taken about 17 million years; but we shall see that there are other causes which may very much reduce the time required; and the vast thickness of calcareous rocks, which are only the record of others from which they were partly formed, shows how many times such areas must have been transported from land to sea. And it must not be forgotten that the configuration may have been more favourable, though the present allows us to demonstrate that this may be a disturbing cause, and that there will be a tendency for the oceanic half of the globe to gain weight over the land-hemisphere. Having examined our first external cause, we turn to the displacement of the water. The sinking of an area equal to the continent of Asia to the mean depth of the ocean would bring a weight of water sufficient, if the antipodes were a suboceanic rising area, to displace the position of the axis 40–60 miles.

I may here say that the amount of change I have supposed is not more than has taken place since the beginning of the Miocene period, though of course not in the manner put; and we see that at present, while land-areas, as part of

vation- to the submergence-area on one side, and the opposite points from the submergence- to the elevation-area, so that land antipodal to land, and water to water, is the consequence.

Australia, South America, the north of Europe, are rising, a vast area in the Pacific is now sinking.

There are other causes which would increase the effect we have seen to be possible from denudation by solution; for the amount must be added which the ocean will dissolve from the matter brought into it as detritus and coast-waste. The solid amount the rivers remove in suspension will be about six times that removed in solution*; and we shall perhaps not be far wrong if we suppose that coast-waste removes a mass of rock equal to the amount removed in solution by the rivers. This weight of detritus and coast-waste of about half a Vesuvius is rolled about by the ocean and exposed to the solvent action of the surplus amount of carbonic acid, and thus the amount in solution in the ocean is largely increased.

The transfer of detritus may, with a certain distribution of land and sea, remove the weight very unequally; and if we turn to Asia, we see that, with the heavier rains in the south and the sluggish northern rivers, and the great chain of the Himalayas draining to the south from both slopes, most weight is taken to the south; but this could not be a very important item.

Another cause of irregularity that should also be taken into consideration is, that the amount of evaporation being greater in the warmer regions will cause a greater amount to be brought to the tropics than is taken away by currents, and thus more matter in suspension will be transported here than to other zones; and this, with an unsymmetrical division of land and sea, would exert an appreciable effect.

Many geologists believe that there has been in quite recent geological periods a much greater rainfall†.

Making allowance for the amount dissolved by the ocean

* T. M. Reade, *loc. cit.*

† See Prestwich, Anniv. Address Geol. Soc. 1872; and Sollas, "On Evolution in Geology," Geol. Mag. Jan. 1877.

being about equal to that brought down by the rivers in solution, allowing for a greater rainfall and melting of a considerable amount of ice, we should scarcely be able to get by the first cause more than 18–27 miles in a million years; and the second might give 40–60 miles within very recent geological periods.

We thus see that, should these causes not have received full allowance from Mr. G. H. Darwin, the amount of change of 1°–3° is nearly all that could be allowed for any geologically recent change, unless any agency could be found which might work in a cumulative manner so as to increase the effect.

Although we must suppose that the change would take place very gradually, the effects to be appreciated must be considered as if they were brought about suddenly. Suppose that the angle of the axis is changed one degree, the earth as a yielding body must take the new figure of rotation. Taking that hemisphere in which the bulge is removed towards the south, we have an undue amount of material in the south; and this must cause phenomena which would be equivalent to an elevation in the north, displacing, if it should be an oceanic area, an amount of water to be placed in another region. The direction may be reversed in consequence of the unequal length of the minor axes. There is a difference in the equatorial diameters; the shortest diameter is that drawn from somewhere about the north of South America to the antipodes north of Australia, the difference being from one to two miles.

Further, the differences of the land-diameters are much greater, so that the equatorial diameter of the portion of greatest specific gravity (covered by the ocean of less) is in some parts 5–6 miles less than the diameter of the figure. The maximum effect which would be theoretically possible for each degree of change, allowing for the dif-

ference of specific gravity of water and earth, is only what could be exercised by $\frac{1}{212}$ of the weight of the equatorial bulge.

Were the earth not a yielding body, it is apparent that any great alteration of the position of the axis would materially affect the tides in a way which is not indicated by any geological phenomena. Prof. Twisden pointed out at the Geological Society that an alteration of 20 degrees with an unyielding globe would cause a tide seven miles high.

Were the earth perfectly rigid, the greater denudation would naturally take place from the area of undue bulge, and so the figure of rotation might be ultimately restored. May not readjustment account for a land-hemisphere such as we have at present?

It may be objected that the changes we have been examining in detail could never act more than temporarily in one direction, and, by changes in the land and sea, the water which had been removed would be brought back, and the axis would take its old place. This is not at all a necessity; for supposing, after the large continental elevation which was caused by the earth's taking the figure of rotation, and which would have an axis of elevation east and west, that we now have water-troughs and minor elevations equally distributed north and south, this is what theory would expect, and which we find on the smaller scale on which it has been studied. In this way it might be made an oceanic area and be now ready for work again. I commend the consideration of the effect of the readjustment of the earth's crust to the consideration of those able to grapple with it.

It will be objected that any such movement of the axis would be discoverable astronomically from the earth- and moon-motions—that is, by the precession and nutation of the equinoxes, which are caused by the attraction of the

sun and moon on the mass of the equatorial bulge. It is from no sufficient change having taken place that we have been told the figure of rotation has not altered in 3000 years (the limit of known observation); but this has been based upon a preternatural rigidity of the earth. Now, although we see that the earth does not instantaneously take its new figure after any change, or else every mountain would be levelled in twenty-four hours, yet the changes must be taking place so gradually and the earth readjusting itself so imperceptibly, that I do not know if observation ought to be able to discover any such small changes in the precession and nutation. Further, we must not forget that in many conditions of configuration the forces working to move the axis in one direction would be balanced by those in the other; and this may have been the condition of the earth during the last 3000 years. Geological evidences show, from the smallest scale to the largest, that the earth is not an unyielding body. This the plications and foldings of the hardest rocks show, and the manner of elevation of continental areas: this has not always been allowed by physicists. But now Sir William Thomson, in his address to the Mathematical Section of the British Association, 1876, said, "were it of continuous steel and 500 kilometres thick, it would yield as much as if it were india rubber to the deforming influence of centrifugal force, and of the sun- and moon-attraction"*.

* It will be seen that it is not the rigidity, as bearing upon the question of tides or of a fluid interior, that is under consideration, but how far the earth is a yielding body—whether it may, "as a whole, be stiff enough to practically fulfil the condition of unyieldingness." To which Sir William Thomson answers, "No, decidedly it would not." It is true that, with a solid globe, the deforming influence which a fluid interior would give by the outward pressure of this fluid, caused by the sun and moon's attraction, would be wanting; yet the centrifugal force, which is always acting in the same direction, and not intermittently, as tidal periods, would alone, with a yielding body, seem sufficient to change the figure.

We have seen how the forces under consideration may have acted in opposite directions; and a consideration of the recent geological phenomena shows that, while large areas have been elevated, it has not taken place steadily and uninterruptedly, but that there have been elevations and subsidences many times repeated*; so that if we turn to the north of Europe, in Belgium, or to Italy, we find, for a general elevation of a few hundred feet to have taken place since the middle of the Tertiaries, there have been subsidences and elevations of many thousand feet in each direction. Now, with the number of forces at work, and the irregular distribution of land and sea, we may say that a reversal of condition in one part could not take the axis back to the same place; in other words, the poles might tack.

Might not in this way a cumulative effect be given to those alterations which it has been shown elevation of large areas can bring about?

As this is a question for astronomers and physicists rather than geologists to investigate, and they may not so fully appreciate the changes that have taken place in the Tertiary period, it may be advisable to glance at the main phenomena. After the Cretaceous period, the Alps, Andes, Pyrenees, and Himalayas have been elevated, partly in the Eocene, but mostly in the Miocene and also Pliocene. In the Miocene the sea covered more than once the north of Europe, the centre of France, nearly all Italy, and a great part of the rest of South Europe, and a large part of North Africa, a great part of India and South Asia, the centre and east of Australia, much of North and South America. Elevation took place, and also subsidences; so that we find during part

* Some similar phenomena may have been caused by alteration of the water-level and not the land; but for our present purpose we have nothing to do with the cause.

of the Pliocene a great portion of the same area was covered; and it was only in this period that most of Italy became land. And then came the ice-age, when the ice must have increased gradually, and the glaciers grew until those from England and Scotland covered a considerable part of Britain and joined those from Scandinavia, and, as all know, the glaciers of the mountain regions of Europe, Asia, and America had such great extensions. During this and the interglacial period elevations and subsidences (or kindred phenomena) were brought about, in some places amounting together to not less than four thousand feet.

It would be almost impossible to conceive that the changes since the Eocene were all accomplished in a million years; that would seem a short time for such alterations, which have taken place since so recent a geological period. But geologists must not claim more than a minimum; for Sir William Thomson a few years ago pulled up some rather sharply for going too fast, and making too great calls upon time, which it had become rather the custom to look upon as unlimited. By various methods it was indicated that the time to be allowed for life on the earth could not have been more than about 100 million years. The only argument which now remains is that which limits the age of the sun's heat, which must be definite when a complete knowledge is obtained of the method in which the heat was generated.

I have not entered into a consideration of the effect of upheaval on the centre of gravity and the difference of contraction of the earth's crust by secular cooling under the land- and water-areas, the production of which may be influenced by the position of the earth's axis; but I hope some Member of this Society may take up these questions.

The points I wish to bring forward are, that a change of the position of the axis would elucidate many facts which

have not yielded to any other explanation, and that a change of the position must take place, but that the amount required is greater than the changes since the Miocene would lead us to anticipate as caused in a direct manner; but if there is a cumulative effect, then it may be explained.

The idea thrown out for examination is, whether the frequent changes in direction, which are caused by a force working in various parts of the world, would not thus give a sufficient increase to the amount calculated.

XVIII.—*Losses and Gains in the Death-toll of England and Wales during the last Thirty years.* By Arthur Ransome, M.A., M.D.

Read March 20th, 1877.

In a paper recently read before this Society* Mr. Baxendell endeavours, with much research and ability, to reckon up the gains and losses in the mortality of England and Wales from different diseases. If the records upon which he bases his conclusions had been entirely trustworthy, the results which he has obtained would, doubtless, have been valuable, and might, perhaps, have indicated the presence of certain influences bearing upon the public health; but even so, I venture to think that the figures he adduces would not bear out the inferences which he attempts to

* "On Changes in the Rates of Mortality from different Diseases during the Twenty years 1854–73," *suprà*, pp. 61–73.

draw from them. Unfortunately, the returns of the Registrar-General cannot be relied upon for the species of information which he seeks to obtain from them.

Even when it is required only to estimate the gross mortality of various places, there are important limitations to their usefulness, owing to the various nature of the boundaries of the several registration-districts, the presence or absence of hospitals, and the movements or relative composition as to age &c. of the populations. Carefully collated data on these several points are given in the later reports; and by dint of much labour some of the more serious errors from these causes may be avoided; but when we come to the nosology of the tables and try to pick out, as Mr. Baxendell has done, the causes of the mortality in different years, we are at once foiled of accurate results by the imperfections of the records and the impossibility of verifying the causes of death.

These drawbacks to the utility of the returns have often been pointed out; and in the early reports of the Registrar-General, Dr. Farr himself complains of the imperfect nomenclature of disease. Doubtless some improvement has taken place of late years; but such variations in the accuracy of diagnosis at once prevent any value from attaching to a comparison between earlier and later records. Until a very recent period the registration of deaths was not compulsory at all; and in the Report of the Medical Officer to the Privy Council for the year 1864 it is noted that, out of 500 deaths registered in St. David's district, only 15 were recorded by properly qualified medical practitioners. To the present time it is highly probable that the disease Phthisis or Consumption is far too often recorded as the cause of death in some districts in Wales, since it is made to include many forms of disease that cause wasting before they cause death.

Dr. Haviland's recent map of the distribution of phthisis thus makes it appear that the Island of Anglesey is one of the worst districts in the kingdom for the production of this disease, the great probability being that it is no worse than its neighbours.

It is most likely owing to this imperfection of nomenclature that changes have taken place in the apparent rates of mortality from some of the diseases mentioned by Mr. Baxendell. Many of the deaths recorded of late years as arising from bronchitis would probably have been set down in former times to pneumonia; and hence the former disease appears to have increased in fatality, the latter to have diminished. Hence also, owing to an improved diagnosis, the term dropsy will have given place to the name of the organ out of whose derangement it has arisen, and we have it less frequently recorded as the cause of death, whilst diseases of the heart, liver, and kidneys have apparently increased in prevalence.

The vague terms brain-disease, cephalitis, and convulsions may also have undergone some amount of transposition.

But Mr. Baxendell points out a very great increase (86 per cent.) in one disease, small-pox, that is not likely to have been mistaken for any thing else; and he further shows that this increase has taken place mainly amongst the adult members of the population.

His remarks on this point are undoubtedly very important, and will need to be well weighed by those who are responsible for the efficient performance of vaccination.

It is important to observe, however, that the whole of this increase occurred in the years 1871 and 1872, when over 40,000 deaths took place from this cause.

The period of ten years chosen by Mr. Baxendell is probably not long enough to afford a basis of argument respecting epidemic diseases; forty or fifty years are sometimes

required to contain the full cycle observed by some of them, as may be seen from the diagram showing the variations in the prevalence of scarlet fever in Sweden.

The cycle usually followed by small-pox, however, is shorter than this; and from the exemption of children from the excessive death-rate by this disease it would seem that the protection afforded by infantile vaccination endures now for a shorter period than it formerly did.

This surmise is in accordance with the facts recorded by Mr. Simon respecting the increase in the number of cases of successful revaccination in the Prussian army during the years 1813 to 1833.

It points to some imperfection in the operation, or to a possible deterioration in the quality of the lymph, owing to its transmission too constantly through the human subject. In any case, its cause is worthy of full and patient investigation.

The objections that I have ventured to bring against the details of Mr. Baxendell's tables also do not apply to his comparison of the gross mortalities of the two periods he has chosen.

The almost stationary condition of the death-rate of the country during the last thirty years has been noticed by many who are interested in the public health, and who have been disappointed at not obtaining from the Registrar-General's tables of total mortality that proof they so much desired of the efficacy of their sanitary improvements. Mr. Baxendell, indeed, shows that all the zymotic class of diseases, with the single exception of small-pox, have somewhat diminished; and by limiting the term "preventible" to this class of disorders he is able to ascribe to sanitary measures the modicum of improvement represented by "a saving of one life in every 134," or "a reduction of 0·17 per cent. in the general death-rate."

At the risk of sacrificing even this small tribute to the value of sanitary work, I must, however, point out that this restriction of the term "preventible" is not usually recognized by the medical profession. The sanitarian claims much more than epidemic diseases as truly preventible. The influence of favourable or unfavourable sanitary conditions is also plainly shown in such cases as phthisis, diarrhœa and dysentery, scrofula, various pulmonary complaints, convulsive disorders, and, to some extent, in the whole range of diseases that produce our excessive infant-mortality. The true meaning of the term "preventible" mortality is fully given by Mr. Simon in his preface to Dr. Greenhow's Inquiry. Briefly stated, it means "all local excesses of fatality that are due to local circumstances of aggravation" (p. viii).

The excessive mortality from a large group of diseases other than epidemic in their character may then be greatly diminished, partly by public sanitary work, and still more by improvements in domestic hygiene amongst the poor—such as sanitary associations and medical officers of health may by degrees be able gradually to teach.

It may fairly be asked, then, of sanitarians, where is the proof of your ministry?

The range of preventible mortality is greater than Mr. Baxendell supposes; but it has not been prevented. To what must this result be attributed? Mr. Baxendell seems inclined to question the utility of the costly sanitary works that have been undertaken of late years; but an examination of the local details that make up the total mortality will soon show that in places where these works have been thoroughly carried out a very distinct effect upon the mortality has been produced, and that the stationary condition of the total death-rate is mainly caused by the increasing mortality in places that have not been thus cared for.

It is indeed difficult for any one acquainted with the history of the science of public health to see the necessity for any defence of sanitary works, seeing that in some form or other they have led to the entire disappearance of some diseases and to the great amelioration of others, and that where they have been thoroughly carried out they have had a distinct effect either in directly diminishing the death-rate or in counteracting a tendency to increased disease and death.

The Registrar-General in his last report gives a few examples of places in which there seems to have been a direct connexion between sanitary improvements and diminished rates of mortality. His list is as follows; but the time at my disposal does not permit me to append his remarks as to the kind of works executed at each place:—

Name of district.	County.	Population.		Average annual mortality.		
		1861.	1871.	1841–50.	1851–60.	1861–70.
North Wichford..	Cambridge	14791	15585	27	21	20
Whittlesey.....	„	6966	7002	25	23	21
Wisbeach	„	33309	34209	25	22	20
Ossett.....	Essex	11595	13172	24	21	18
Salisbury	Wilts	9039	9212	28	24	20
Stoke Dameral ...	Devon.....	50440	49449	26	23	21
Wolverhampton..	Stafford ...	126902	136053	27	28	24
Coventry	Warwick..	41647	40113	27	25	21
Macclesfield	Chester ...	61543	59339	26	25	23
Hull	York	56888	68316	31	25	26
Newport.....	Monmouth	51412	61252	24	22	21
Merthyr Tydfil ...	Glamorgan	93008	104239	28	29	25
Crickhowell	Brecknock	22457	20147	27	25	23

But it may be said that of late some form of sanitary administration has been at work all over the country, why then have recourse to detached examples? The reply is sufficiently simple, that this universal sanitation has certainly not yet had time to prove its power for good.

I should be very unwilling to defend the present chaotic, inefficient, and extravagant method of carrying on public-health government. Under it the local medical officers have no security of office; many are appointed over petty areas upon paltry salaries, the aggregate of which would produce a good income for well-trained men. Most of them have had no special training; and none of them have any means of proving their capacity by a state qualification. They have no well organized system for obtaining immediate information of the outbreak of disease; and, from the want of proper instruction and central assistance, they are driven to make experiments in sanitation which, by their failure or by their cost, raise a formidable local opposition to all their efforts.

It will be seen then that I entirely agree with Mr. Baxendell in thinking that "the present sanitary system is very defective;" but it by no means follows that it has done no good.

In auditing carefully the several items of a profit-and-loss account, there may well be found an equipoise between the two sides of the account, or even a loss on the whole, and yet it may be possible to see that, unless certain adverse circumstances had occurred, such as a rise in wages, increased price of raw material, &c., the result would have been a decided gain.

The general death-toll of a country is in truth under the influence of many most various conditions. It is only possible to discover the full effect of sanitary measures when all these conditions are taken into account.

It would be a weak form of defence for these measures if we were to say that if they had not been taken "things might have been worse;" for they might, perhaps, even have been better. But the argument changes its character altogether if it can be shown that the death-rate certainly

would have been heavier had not sanitary or other influences been active to counteract the increase of morbific agencies that have been at work.

It is not difficult to prove that certain important causes of excessive mortality have of late years increased considerably in their pressure upon the populations of England and Wales; and I would specially advert to three of these:—1. The density and size of towns; 2. Intemperance; 3. The improper feeding and treatment of children.

1. *The Density and Size of Towns.*

"In the last twenty years," says Dr. Farr, "the towns of England have increased from five hundred and eighty to nine hundred and thirty eight, their population from nine to fourteen millions."—Supplement to 35th Ann. Rep. p. viii.

The population throughout the country has increased so that, on the average, there are now 1·74 acre to each inhabitant; and in the decade 1841–50 there were 2·21; and in most of the towns there is a still greater proportional increase in the density. Thus in London it has doubled, the acreage to a person sinking from ·04 to ·02; and the same proportional decrease of acreage is found at Plymouth (·04 to ·02); at East Stonehouse it has sunk from ·02 to ·01; in Salford and Portsmouth the space has diminished from ·08 to ·05, in Leicester from ·07 to ·04, in Stoke-on-Trent from ·23 to ·15, in West Bromwich from ·32 to ·19.

It is not easy to estimate the exact effect upon health of this crowding together of populations. Its operation is compounded of many distinct elements. Thus Dr. Farr himself ascribes a direct influence to the dead and decomposing organic matter floating in the air of towns, the product of respiration and of excrementitious matter of all kinds; and there can be little doubt that it is a powerful

predisposing cause of the large group of scrofulous diseases that annually carry off more than 30,000 persons.

But in towns there are found many other malign influences in full operation in much greater abundance than in the country—ill-ventilated, unhealthy, crowded dwellings, sedentary and unwholesome trades and occupations, together with much freer play to the two other causes of increased mortality that have been mentioned, such as increased facilities and temptations to the use of strong drink, female employment, and consequent neglect of children by their mothers.

That town-life is more unhealthy than country-life, that the mortality increases in some way with the density of the population, is sufficiently shown by the following Table :—

Mean annual death-ratio.	Density. Persons to a square mile.	Births.	Deaths.	Annual increase of population.
15 39	315	35·11	22·00	11·99
15 17	166	30·22	16·75	14·69
18 20	186	32·19	19·16	7·53
21 23	379	35·78	21·88	13·82
24 26	1718	38·75	24·90	18·59
27 30	4499	40·16	28·08	13·28
32 (Manchester)	12375	37·33	32·49	3·22
39 (Liverpool)	65823	37·57	38·62	−12·23

The law of increased mortality with increased density of population is, on the large scale, so definite and certain that Dr. Farr has been able to construct a formula by which in many cases the mortality can be calculated from the density; and if accurate data were obtainable, it might even be possible to ascertain what increase of mortality might have been expected to result from the increased tendency of populations to aggregate themselves together.

We cannot state the problem quite so definitely as this would imply; but if we take Dr. Farr's estimate of 5 millions of additional inhabitants in our towns, and apply to this the usual urban rate of 24 as distinguished from the rural rate of 19 per 1000, we obtain an excess of 25000 deaths that would have to be placed to the account of overcrowding.

The fact that, on the whole, the mortality has remained stationary during the last thirty years, shows that 25000 lives must have been saved by other means, and hence that some influences have been at work to mitigate the evil effects of overcrowding. What these influences are is made at least very probable by the fact that, in most of the notable exceptions to Dr. Farr's law, sanitary improvements have been carried out for a good many years. In the following examples it will be noted that the density of most of the places named has decidedly increased, whilst the mortality has diminished.

Districts.	Density. Acres to a person.			Death-rates.		
	1841–50.	1851–60.	1861–70.	1841–50.	1851–60.	1861–70.
Salisbury	·05	·05	·05	28	24	20
Wolverhampton	·58	...	·42	27	28	24
Coventry.....	·16	...	·13	27	25	21
Macclesfield ...	1·37	1·31	1·36	26	25	23
Newport.....	2·88	...	1·96	24	22	21
Merthyr Tydfil	1·74	...	·71	28	29	25
Gloucester	1·09	·96	·84	24	22	22
Leicester.....	·07	·06	·04	27	25	26
Clifton	·41	·34	·26	23	20	20
Salford	·08	·06	·05	28	26	27
Cheltenham ...	·59	·53	·50	20	19	19
Altrincham ...	2·26	1·98	1·63	23	23	21
Newport.....	3·13	3·06	3·03	20	20	18
Dover.....	1·13	1·00	·87	21	20	20

The first six of these districts are taken from the Table

already quoted from the Registrar-General as having improved as the result of sanitary works; and in each of the remainder it will be found that similar works have for some time been pursued.

2. *Intemperance* has been assigned as an increasing cause of mortality during the last thirty years. There is no doubt as to the increased consumption of alcoholic drinks of late years. "In the five years ending 1845 the average expenditure of the United Kingdom upon these drinks was about 77 millions sterling, whilst in the five years ending 1875 it was 135 millions."—*W. Hoyle.*

It is difficult to learn how much of this alcohol has been taken in excessive quantities, and how much has been added in small amounts to the diet of those who formerly could not afford to take any. Many persons, doubtless, would come under the last-named category. During the last twenty years trade has, on the whole, improved. There have been higher wages paid; and a portion of these will have been spent merely in dietetic drinking; but it is certain that there has been a large increase in the numbers of those who drink to intoxication. "In 1860 the apprehensions for drunkenness were only 88,000, in 1875 they were 203,000."—*W. Hoyle.*

This increased intemperance must have shortened many lives. To repeat here Mr. Neison's table, a temperate person's chance of living as against that of an intemperate person is, at the age of twenty years, 44·2 years to 15·6 years; at thirty, 36·5 years to 13·8 years; at forty, 28·8 years to 11·6 years; at fifty, 21·25 years to 10·8 years; at sixty, 14·28 years to 8·9 years. Of intemperate persons the mortality at twenty-one to thirty years of age is five times that of the temperate; from thirty to forty it is four times as great.

Since alcohol affects chiefly the liver, kidneys, and heart

we may, with much probability, assign the great increase in diseases of these organs, pointed out by Mr. Baxendell, mainly to this cause, and not to defective sanitary administration.

It is of course impossible to say how much of the mortality of the country is due to this cause; but I will mention one instance in which it would appear that some approximation may be made.

My friend Dr. W. Roberts informs me that about thirty years ago a very powerful temperance movement affected large districts of Wales to such an extent that in Anglesey most of the public houses were closed and a large proportion of the people became total abstainers from alcohol. This movement had, to a great extent, died out by the year 1850.

Now, the density of the population in this county (of Anglesey) has remained almost stationary; but its mortality has increased, as shown by the following Table:—

Density and Mortality in the Island of Anglesey in Three Decennia.

District.	Density. Persons to a square mile.			Annual mortality. Deaths to 1000 living.		
	1841–50.	1851–60.	1861–70.	1841–50.	1851–60.	1861–70.
Anglesey.....	3·41	3·16	3·36	17	19	21

Since the population of these districts has certainly improved in material comfort in the last twenty years, it is probable that a large part of the increased mortality may be fairly ascribed to the increased use of alcohol. A similar statement holds good with regard to other parts of Wales.

If it were allowable to extend the analogy of these places to the whole of England, the stationary condition of the death-rate of the country would have to be accounted for by discovering some causes that had diminished the death-rate by at least two or three per thousand, and had saved about 40,000 or 50,000 lives every year.

I cannot help thinking that in many parts of England this statement falls short of the truth, and that we have to thank sanitary effort for saving the country from a still greater loss.

3. The third element in the positive increase of the death-rate is the *infant mortality*. Now this I have already included to some extent under the head of "preventible mortality;" but whilst it is, perhaps, more under the control of proper hygienic conditions than any other form of death, these conditions are rather to be ranged under the subtitle of domestic arrangements than as pertaining to the ordinary sanitary work of local authorities. It is less likely than adult mortality to be at once affected by the more common mechanical improvements of districts, such as improved sewage and good water supply; and it requires for its amelioration better house accommodation, more efficient house-drainage, a freer supply of pure air, cleanliness, a proper supply of suitable sustenance, and adequate protection against infection.

It cannot be said that these points have yet received full attention from the sanitary administrators of the country; and hence the death-register of infants can hardly be quoted as evidence of their failure.

On the other hand, children are more exposed than adults to the already-mentioned causes of ill health: they are more directly affected by over-crowding; and they are the first to feel the effects of the neglect arising from intemperance. This indirect consequence of the abuse

of alcohol is probably still more fatal than its direct influence.

It is probable that the mode of feeding young children, especially in the manufacturing districts, is becoming less and less adequate to their needs.

The inquiries made by Dr. Ferguson, of Bolton, and others, show that tea and many other forms of experimental foods are now tried upon children instead of the milk that was formerly more easily procured, and that the result is a gradual falling-off in the physical power of our manufacturing populations.

There can then be no doubt that the adverse influences bearing upon infant life have been increasing in intensity during the last twenty years, and they have not failed to produce their effect.

Notwithstanding the effects of overcrowding and intemperance, the adult mortality of the country, owing to some beneficent cause or other, has, on the whole, slightly diminished; but the mortality under five years of age has increased.

The following Table (p. 140), kindly prepared for me by Mr. Royston from the Returns of the Registrar-General, shows this in a striking manner, the adult mortality having lessened ·17 per 1000, the infant mortality having increased 1·50 per 1000 in the fifteen years ending 1870.

I think it will be acknowledged that it is not easy for sanitary science to struggle against the hostile forces that have now been enumerated. If they had not been met by measures to some extent competent to mitigate their power, the condition of the country would have been perilous in the extreme; and at the present time we are by no means free from serious danger, owing to their increasing pressure—danger of epidemic disease fostered by the organic matter generated by overcrowding, danger from intemperance to

both national health and national wealth, and danger of deterioration of race owing to the sickliness and stunted growth of the children that survive. It is surely our duty, then, to promote a more efficient system of sanitary administration, to take full advantage of the facilities afforded by recent legislation for the improvement of our towns, to try to abate the practice of intemperance, and to endeavour to spread amongst all classes a knowledge of the laws of health.

Mortality in England and Wales.

Years.	Total mortality per 1000 of population.	Average.	Mortality under 5 to population at same ages.	Average.
1838 to 1840	... 22·4 ...	18 years. 22·47	... 66·55 ...	18 years. 66·66
1841 to 1845	... 21·4 ...		... 63·9 ...	
1846 to 1850	... 23·35 ...		... 67·0 ...	
1851 to 1855	... 22·7 ...		... 69·15 ...	
1856 to 1860	... 21·85 ...	15 years. 22·3	... 67·25 ...	15 years. 68·16
1861 to 1865	... 22·6 ...		... 69·05 ...	
1866 to 1870	... 22·45 ...		... 68·2 ...	

XIX. *List of Desert-Plants collected at Ramleh, near Alexandria, Egypt, from September 1875 to April 1876.*

By H. A. Hurst, Esq.

Read before the Microscopical and Natural-History Section January 15th, 1877.

In placing before the Section a list of the plants collected during the winter of 1875–76 at Ramleh, Egypt, I may remark that there is another place of the same name in Palestine, between Joppa and Jerusalem. In both cases, I believe the name to be derived from the Arabic R-M-L, signifying sand, its derivative being used to express a desert.

It is a narrow slip of undulating ground between the Lake Maadieh on the right hand, looking north, and the sea on the left. It abuts on the ground where Cæsar had his camp, B.C. 48, remnants of which remained till lately, when its traces were almost completely effaced by Ismael Pasha, the stones being taken to build one of his many palaces, a very striking building, at present uninhabited, and one of Egypt's many monuments to waste.

Ramleh is interesting to Englishmen as the site of the battle of Alexandria, in 1801, when the English, under Sir Ralph Abercromby, defeated the French, who were then in possession of Egypt. It will be remembered that the leader fell, mortally wounded, in the action.

At the present moment the place bears much the same relation to Alexandria which Bowdon does to Manchester, or Richmond to London. There is a railway running to it from Alexandria, the furthest station being about five miles distant from Cleopatra's Needle; and the ground is

sprinkled over with well-built villa residences, to which English, Greek, Italian, and other residents of Alexandria resort during the warmer months of the year, some even making it their permanent abode. The dwelling-houses cease entirely about six miles from Alexandria, where uninhabited desert is reached. Botanically speaking, the district may be divided into three portions—the sea-coast, the desert, and the irrigated margins of Lake Maadieh. The plants, of which the following is a list, were collected in the two former districts. I hope, on a future occasion, to bring before you those found on the irrigated land in the vicinity of Ramleh and Alexandria generally.

It is stated that the migratory birds pass from Europe to Africa and Asia by three routes, the most westerly being by Gibraltar, the central by Malta, and the most easterly by Greece, Crete, and Egypt. I am inclined to think that the point for which these last birds direct their course is Ramleh, the coast being comparatively conspicuous, both from its height and from the waves generally breaking on its shores. This may possibly account for some of the plants there found.

According to Boissier, one of the most ancient botanical explorations was that of Prosper Alpinus, in the sixteenth century; Lippi also travelled in the country at the commencement of the eighteenth century; but although it may be true that

"Vixere fortes ante Agamemnona,"

still it is from Linnæus and his followers that we, in the present age, date our knowledge.

Forskåhl (a pupil of Linnæus) arrived, in 1761, in Egypt; and it was from his 'Flora Ægyptiaco-Arabica' that Europe first became acquainted with the Egyptian flora. He appears to have been a most extraordinary man. He was robbed and ill-treated, and, it is reported, sold as a

slave in Egypt. Though suffering from constitutional timidity, and often nearly broken down by sickness, the extraordinary energy and perseverance with which he examined the vegetation of Egypt and Arabia produced a work unsurpassed in its day. The hardships he underwent prevented his surviving to publish it. We are indebted to his friend Niebuhr for its appearance; and it is remarkable as being one of the first works in which the relation of vegetation to climate is taken as an object of importance. For many years the 'Flora Ægyptiaco-Arabica' was the only good account we had of the plants of those countries. In the following List I have aimed at giving his synonyms of all the plants enumerated.

Delile subsequently published, in the 'Botanique de l'Expédition d'Egypte,' a very valuable account of its vegetation, with excellent engravings.

In later years Ehrenberg and Hemprich, Dr. Rüppell, Aucher Eloy, Bone, Schimper, Boissier, Kralik, and more recently Samaritani, Cadet de Fontenay, Dr. Gaillardot, and Schweinfurth have examined this district.

It does not appear to have attracted the attention of any Englishman except Henry Calvert, Vice-Consul at Alexandria, to whom I am greatly indebted for assistance.

Judge Letourneux, of the new Courts in Alexandria, an able botanist from Algeria, has this year, I believe, taken to Paris a very complete list of the Ramleh plants.

Meanwhile Dr. Oscar Schneider has published an interesting, though incomplete, list in the 'Isis,' Dresden, Heft iii. 1871.

The authority on the subject today, however, is Boissier's 'Flora Orientalis.' The first volume, printed in 1867, hardly does justice to the Egyptian flora; but the subsequent volumes leave nothing to be desired, forming a magnificent work worthy of any age or country.

RANUNCULACEÆ.

Ranunculus asiaticus, L.

Adonis dentata, Delile.

Hab. Northern Africa.

Nigella divaricata, Beaupré, in DC.

N. arvensis, var. γ *divaricata*, Boiss.
N. arvensis, var. *glaucescens*, Gussoni.

Delphinium nanum, DC.

D. perigrinum, Delile, non L.
D. grandiflorum, Forsk., non L.
D. Forskahlei, Reich. Acon. Illust.

Obs. My specimens are from H. Calvert, Esq.

Hab. Alexandria; Hellespont.

PAPAVERACEÆ, DC.

Papaver Rhœas, L.

Papaver dubium, L.

Papaver hybridum, L.

Obs. The above three species all grow on cultivated ground.

Rœmeria orientalis, Bois.

R. hybrida, var. β *eriocarpa*, DC.
R. Schimperi, Presl.
Chelidonium dodecandrum, Forsk.

FUMARIACEÆ, DC.

Hypecoum imberbe, Sibth.

H. patens, Willd.

Hab. Egypt; Cyprus; Palestine.

Fumaria parviflora, Lam.

Fumaria micrantha, Lag.

With its variety β *Parlatoriana*.

Fumaria judaica, Boiss.

Obs. I have followed Boissier regarding this genus, no being satisfied with my own identification of the species.

CRUCIFERÆ, Juss.

Koniga maritima, L.

Koniga arabica, Boiss.

Hab. Alexandria; Borders of Palestine and Arabia.

Biscutella Columnæ, Tenore, var. β *depressa*.

B. depressa, Willd.

Hab. Alexandria.

Capsella procumbens, L.; Boiss.

Cakile maritima, Scop., var. β *integrifolia*.

Hab. Alexandria; Black Sea.

Malcolmia pygmæa, Delile.

Hab. Alexandria; Aboukir; Persian Gulf, on the Island of Karrak.

Mathiola humilis, DC.

M. acaulis, Balb.

Hab. Alexandria; Rosetta; Aboukir.

Obs. Very sweet-scented, especially in the evening.

Enarthrocarpus strangulatus, Boiss.

Hab. Alexandria; Arabia Petrea.

Erucaria microcarpa, Boiss.

Hab. Near the Dead Sea; Alexandria.

Diplotaxis Harra, Forskal.

Carrichtera Vella, DC.

Vella annua, L.

Obs. I found one specimen in 1871. Cadet de Fontenay also records it from Alexandria.

Brassica Tournefortii, Gou.

CAPPARIDEÆ.

Cleome pentaphylla, L.

Obs. Only on irrigated land. Evidently introduced.

RESEDACEÆ, DC.

Reseda alba, L.; H. Calvert.

Reseda propinqua, R. Bro.

R. decursiva, Forsk.

CISTINEÆ, Juss.

Helianthemum kahiricum, Delile.

H. Lippii, L., var. β *Ehrenbergii*.
H. Ehrenbergii, Willk.

CARYOPHYLLEÆ.

Silene canopica, Delile.

Hab. Alexandria.

Silene bipartita, Desf.

Silene succulenta, Forsk.

Hab. Crete; Egypt; Palestine.

Alsine procumbens, Vahl.

Spergularia diandra, Guss.

Arenaria salsuginea, Bunge, in Ledeb. Fl. Alt.

Spergularia media, Vahl.

Arenaria marina, Smith.

PARONYCHIEÆ, St. Hil.

Polycarpon tetraphyllum, L.

Polycarpon alsinefolium, Biv.

Herniaria cinerea, DC.

Herniaria hemistemon, J. Gay.

H. fruticosa, Delile.

Paronychia argentea, Lam.

Paronychia arabica, L.

Also its variety,

P. arabica, var. *β macrostegia*.

Hab. Egypt; Tropical Arabia (Forsk.).

TAMARISCINEÆ.

Reaumuria mucronata, Jaub. et Sp.

R. vermiculata, L. (ex parte).

Hab. Deserts of Northern Africa.

Tamarix arborea, Bunge.

Tamarix articulata, Vahl.

Thuya aphylla, L.

Obs. Planted by roadsides.

CUCURBITACEÆ.

Bryonia dioica, Jacq.

FRANKENIACEÆ.

Frankenia hirsuta, L., var. δ *revoluta*.

F. revoluta, Forsk.

Hab. Near Alexandria.

Frankenia pulverulenta, L.

MALVACEÆ.

Malva cretica, L.

Malva parviflora, L.

M. mareotica, Delile, in DC. Prod.
M. flexuosa, Horn.

OXALIDEÆ.

Oxalis corniculata, L.

Obs. Introduced. Borders of irrigation.

GERANIACEÆ.

Erodium laciniatum, var. β *pulverulentum*.

Geranium pulverulentum, Desf.
E. Bovei, Delile.
E. pyramidum, Presl.

Erodium malacoides, L.

ZYGOPHYLLEÆ.

Zygophyllum album, L.

Peganum Harmala.

LEGUMINOSÆ.

Ononis vaginalis, Vahl.

O. Cherleri, Forsk., non L.
O. vestita, Viv.

Hab. Alexandria; Antilebanon.

Ononis serrata, Forsk.

Trigonella hamosa, L.

Obs. On cultivated ground.

Trigonella maritima, Delile, var. β *dura*, Boiss.

T. dura, Visiani.

Trifolium Alexandrinum, L.

T. Constantinopolitanum, DC., apud Boiss.

Obs. Largely cultivated in Egypt under the name of bersyn; but apparently nowhere spontaneous.

Trifolium bicorne, Forsk.

Medicago marina, L.

Medicago littoralis, Rohde.

Medicago denticulata, Willd., var. β *apiculata*.

Melilotus sulcata, Desf.

Melilotus parviflora, Desf.

Hymenocarpus nummularius, DC.

Medicago circinata, Willd., var. β.

Lotus argenteus, Delile.

Lotus creticus, Viv., non L.

Lotus corniculatus, L.

Lotus tenuifolius, Rchb., Fl. Exc. 506.

L. tenuis, E. B.

Lotus pusillus, Viv.

Hippocrepis cornigera, Bois.

H. bicontorta, Loisel.

Hab. Alexandria; Arabia Petræa; Ramleh (Palestine); Abuschir.

Astragalus Alexandrinus, Boiss.

Hab. Northern Egypt; Arabia Petræa; Palestine; Damascus.

Astragalus annularis, Forsk.

A. subulatus, Desf.
A. maculatus, Lam.

Hab. Egypt; Arabia Petræa; Askalon; Eastern Algeria.

Astragalus peregrinus, Vahl.

Hab. Egypt; Palestine; Northern Africa.

Astragalus trigonus, DC.

Hab. Egypt; Arabia Petræa.
Obs. Very rare about Ramleh.

Onobrychis crista-galli, L.

Alhaji Maurorum, DC.

Vicia calcarata, Desf.

Obs. Among barley.

FICOIDEÆ.

Mesembryanthemum crystallinum, L.

Mesembryanthemum nodiflorum, L.

UMBELLIFERÆ.

Bupleurum glaucum, Rob. et Cast. in DC. Suppl.

Bupleurum ranunculoides, DC. Prod.

Obs. Boissier calls this *B. heterophyllum.*

Deverra tortuosa, Desf.

Coriandrum sativum, L.

Orlaya maritima, Gou.

Daucus littoralis, var. β *Forskahlii*, Boissier.

Torilis nodosa, L.

RUBIACEÆ.

Crucianella herbacea, Forsk.

Crucianella maritima, L.

COMPOSITÆ.

Calendula parviflora, DC.

C. ægyptiaca, Desf.

Hab. Ægean Islands; Corinth; Alexandria; Palestine.

Calendula parviflora, var. β *suberotris.*

Calendula bicolor, Rafin.

C. arvensis, var. β *bicolor*, DC.

Hab. Yacinthus; Attica; Alexandria.

Inula crithmoides, L.

Pluchea Dioscoridis, DC.

Obs. In ditches, hence not a desert-plant.

Phagnalon rupestre, DC.

Filago spathulata, Presl, var. *β prostrata*.

Ifloga spicata, Forsk.

Trichogyne cauliflora, DC.

Diotis maritima, L.

Achillea Santolina, L.

Anthemis indurata, Delile.

Hab. Peculiar to Alexandria.

Anacyclus Alexandrinus, Willd.

Chrysanthemum coronarium, L.

Senecio coronopifolius, Desf.

Senecio vulgaris.

Echinops spinosa, L.

Centaurea glomerata, Vahl.

Centaurea Alexandrina, Delile.

Centaurea dimorpha, Viviani.

Hab. Egypt; Biskra; Laghouat; Tunis.

Ægialophila pumila, L.

Centaurea mucronata, Forsk.

Picris radicata, Forsk.

P. lyrata, Delile.

Crepis bulbosa, Tausch.

Picridium tingitanum, L.

Hyoseris lucida, L.

Lapsana taraxacoides, Forsk.

Hedypnois cretica, L.

CONVOLVULACEÆ.

Convolvulus althæoides, L.

Cressa cretica.

BORAGINEÆ.

Echium sericeum, Vahl.

Echium setosum, Vahl.

Anchusa Milleri.

Anchusa undulata, L.

Anchusa flava, Forsk.

Anchusa ægyptiaca, DC.

Alkanna tinctoria, L.

Echiochilon fruticosum, Desf.

SOLANACEÆ.

Hyoscyamus albus, L.

SCROPHULARINEÆ.

Antirrhinum Orontium, L.

Linaria Hælava, Poir.

Linaria albifrons, Spr.

OROBANCHEÆ.

Phelipæa ramosa.

P. lutea.

Orobanche speciosa.

LABIATÆ.

Salvia controversa, Tenore.

Teucrium Polium, L.

Ajuga Pseudo-Iva.

PRIMULACEÆ.

Anagallis arvensis, L.

PLUMBAGINEÆ.

Statice aphylla, Forsk.

PLANTAGINEÆ.

Plantago maritima.

Plantago Coronopus.

Plantago albicans.

Plantago Lagopus.

CHENOPODEÆ.

Chenopodium murale, L.

Suæda fruticosa, Forsk.

Salsola Kali, L.

Obione coriacea, Moq.

Caroxylon fœtidum.

POLYGONEÆ.

Rumex lacer, Balb.

Emex spinosus, Campd.

Polygonum equisetiforme, Sibth. & Sm.

Polygonum salicifolium, Delile.

Obs. Oscar Schneider calls this *P. serrulatum*, Lagasca; but I think it is Delile's plant.

THYMELEÆ.

Passerina hirsuta.

EUPHORBIACEÆ.

Euphorbia Peplis.

Euphorbia terracinus.

Euphorbia Peplus.

Obs. I take the identification of the above from Oscar Schneider.

Euphorbia Paralias, L.

URTICEÆ.

Parietaria cretica, Guss.

MONOCOTYLEDONES.

AROIDEÆ.

Arisarum Veslingii, Schott.

Biarum Alexandrinum.

IRIDEÆ.

Iris Sisyrinchium, L.

Sisyrinchium minus, Clus.

LILIACEÆ.

Ornithogalum tenuifolium, Guss.

Obs. Commoner at Mandara than Ramleh, where it is nearly extinct.

Allium roseum, L.

Muscari comosum, Mill.

Obs. Among barley.

Bellevalia flexuosum.

Asparagus horridus.

COLCHICACEÆ.

Colchicum ægyptiacum.

XX. *On some Thionates.* By H. BAKER, Student in the Owens College. Communicated by Professor C. SCHORLEMMER, F.R.S.

Read October 16th, 1877.

HAVING been lately working on these salts, I offer the following observations :—

Barium Dithionate, $BaS_2O_6,2Aq$.—According to Heeren* this salt is soluble in 1·1 part of boiling water; but he does not give the boiling-point of the saturated solution; this I have observed to be 102°, and its solubility at this temperature to be 1 in 0·994 part of water. It is remarkable that the saturated solution of such a soluble salt should boil at so low a temperature. The sp. gr. of the crystals of this salt at 13°·5 is 4·536.

Lead Dithionate, $PbS_2O_6,4Aq$.—"It is very easily soluble in water" (Watts). I find its solubility at 20°·5 to be 1 in 0·869 part of water. Its sp. gr. at 11° is 3·259.

Calcium Dithionate, $CaS_2O_6,4Aq$.—The crystals have a sp. gr. of 2·176 at 11°.

Nickel Dithionate, $NiS_2O_6,6Aq$.—One part of this salt dissolves in 0·897 part of water at 12°.

Magnesium Dithionate, $MgS_2O_6,6Aq$.—According to Watts's Dictionary, it "forms six-sided tables, very soluble in water;" but Gmelin says it forms ill-defined six-sided prisms.

I obtained it in oblique prisms, and found its solubility at 17° to be 1 in 0·692 part of water.

* Pogg. Ann. vii. 172.

Sodium Dithionate ($Na_2S_2O_6,2Aq$).—Its sp. gr. at 11° is 2·175. Watts says "it crystallizes by spontaneous evaporation in large transparent right-rhombic prisms." Gmelin gives several measurements; but they are not sufficient to calculate all the forms from. My measurements show the crystals to be rhombic and the axes to be $\breve{a} : \bar{b} : c = 0{\cdot}9922 : 1{\cdot}0000 : 0{\cdot}5981$. The forms occurring are $_\infty P$, $\overline{P}_\infty$, P, $\overline{P}\frac{1}{2}$, $_\infty\overline{P}_\infty$; and the type is long prismatic, through predominance of $_\infty P$. In these and the following angular measurements the interfacial angles are denoted as in Kopp's 'Krystallographie,' viz. :—

A = the angle of a pyramid, over a vertical edge in the brachydiagonal.

B = the angle of a pyramid over a vertical edge in the macrodiagonal.

C = the angle of a pyramid over a lateral edge.

W = the acute angle in the vertical prism, and the lateral angle in a dome.

Form.	Angle.	Calculated.	Found.	Gmelin.
		° ′	° ′	° ′
P	A	125 45	125 40	125 48
	B	125 18	125 29	125 18
	C	81 11	———	80 18
$\overline{P}\frac{1}{2}$	A	88 39	88 34	———
	B	137 43	137 40	———
$_\infty P$	W	89 33	89 25	89 22 and 89° 36′
$\overline{P}_\infty$	W	62 9	62 3	62 12

Silver Dithionate, $Ag_2S_2O_6,2Aq$, forms according to Watts, right-rhombic prisms, isomorphous with sodium dithionate. Gmelin gives some measurements. I find the axes to be $\breve{a} : \bar{b} : c = 0{\cdot}9884 : 1{\cdot}0 : 0{\cdot}5811$, and the forms $\overline{P}_\infty$, P, $_\infty P$, $\overline{P}\frac{1}{2}$, $_\infty\overline{P}_\infty$, $_\infty\breve{P}_\infty$; the type is very short prismatic.

Form.	Angle.	Calculated.	Found.	Gmelin.
		o ′	o ′	o ′
P	A	126 47	126 51	127
	B	126 6	125 55	126
	C	79 9	79 8	79 10
$\overline{P}\frac{1}{2}$	A	89 54	89 58	90 12
	C	104 58	104 52	———
$_{\infty}P$	W	89 20	89 22	89 8
$\overline{P}_{\infty}$	W	60 54	61 1	———

Silver Sodium Dithionate, $(AgNa)_2S_2O_6,2Aq$, is, according to Watts, "apparently isomorphous with the component salts." I find it to be rhombic with the axes $\breve{a} : \bar{b} : c = 0{\cdot}9813 : 1{\cdot}0000 : 0{\cdot}5856$, and the forms $\overline{P}_{\infty}$, P, $_{\infty}\breve{P}_{\infty}$, $_{\infty}P$, $_{\infty}\overline{P}_{\infty}$. The dome is developed so much as to make the crystals horizontally prismatic; the prism is only slightly developed; and the pyramid $\overline{P}\frac{1}{2}$ does not occur. Watts also says it "exhibits very distinct cleavage;" this cleavage-plane I find to be $_{\infty}P$; and I also find that the sodium and the silver dithionates exhibit very distinctly this cleavage.

Form.	Angle.	Calculated.	Found.
		o ′	o ′
P	B	125 30	125 23
	C	79 48	79 46
$_{\infty}P$	W	88 55	89 1
$\overline{P}_{\infty}$	W	61 39	61 41

This sample contains % Ag 32·0.
$AgNaS_2O_6,2Aq$ requires % Ag 33·00.

Potassium Trithionate, $K_2S_3O_6$.—The following are the statements regarding the crystalline form of this salt:—*Gmelin* gives two:—(1) "slender, four-sided, obliquely truncated prisms" (Plessy, J. für pract. Chem. xxxiii. p. 348); (2) "right rhombic prisms with dihedral summits resting on

the acute lateral edges" (Provostaye, N. Ann. Chim. Phys. iii. 354). According to Watts, it forms four-sided prisms, bevelled with two faces; and according to Miller, it crystallizes in the rhombic system, prismatic type with dihedral summits. I obtained it in rhombic needles of $_{\infty}P$, with the acute edges modified by another prism $_{\infty}\breve{P}2$. $_{\infty}\bar{P}_{\infty}$ and $_{\infty}\breve{P}_{\infty}$ are also present; they are terminated by a brachydome. Some of the crystals taper like a sewing-needle—I suppose, through the presence of a very acute pyramid. I find the axes to be $\breve{a} : \bar{b} : c = 0{\cdot}3586 : 1{\cdot}0000 : 0{\cdot}4204$; and the observed forms are $_{\infty}P$, $_{\infty}\breve{P}2$, $_{\infty}\bar{P}_{\infty}$, $_{\infty}\breve{P}_{\infty}$, $\breve{P}_{\infty}$.

Form.	Angle.	Found.	Calculated.
		° ′	° ′
$_{\infty}P$	W	39 31	39 28
$_{\infty}\breve{P}2$	W	71 12	71 18
$\breve{P}_{\infty}$	W	45 37	45 37

This salt is prepared by acting on a saturated solution of potassium thiosulphate with SO_2. Now, as it is not stated that sodium thiosulphate will not similarly yield sodium trithionate, nor any reason given why it should not, I attempted to prepare sodium trithionate in this manner, but only obtained crystals of sodium thiosulphate. Rathke (Journ. pr. Chem. xcv. 13) prepares sodium trithionate as a white powder by decomposing potassium trithionate with sodium tartrate.

XXI. *On the Formation of Hailstones, Raindrops, and Snowflakes.* By Prof. O. Reynolds, M.A., F.R.S.

Read November 13th, 1877.

My present communication forms a continuation of the paper I read before this Society on the 31st of October, 1876, "On the Manner in which Raindrops and Hailstones are formed."

To the contents of this paper I shall have to refer continually; hence, in order to render what I have to say intelligible, it may be well for me to recapitulate some of the leading points in my former paper. The chief purpose of the paper was to explain the manner in which the minute cloud-particles aggregated so as to form raindrops and hailstones.

Aggregation resulting from the more rapid Descent of the larger Particles.

I commenced by pointing out that, as the suspended particles of water or ice which constitute a cloud are all descending with velocities which increase with their size, the larger particles will descend faster than the others, and will consequently overtake those immmediately beneath them; with these they will combine so as to form still larger particles, which will move with greater velocity and, more quickly overtaking the particles in front of them, will add to their size at an increasing rate. And I then proceeded to consider how far this was a sufficient as well as a necessary cause of the phenomena of hail and rain. One of the most important points on which my arguments were based was

The Shape and Structure of ordinary Hailstones.

On close observation I had found, what had previously been noticed by other observers, that the shape of an ordinary hailstone is not what it at first sight appears to be. They are not spheres more or less imperfect, but more or less imperfect cones or pyramids with rounded bases, like the sectors of spheres—the conical surface being striated, the striæ radiating from the vertex of the cone.

In texture the hailstones have the appearance of being an aggregation of minute particles of ice fitting closely together, but without any crystallization such as that seen in the snowflake; while, on careful observation, it is seen that they are denser and firmer towards their bases or spherical sides than near the vertex of the cone, which latter often appears to have been broken off in their descent.

As I explained, it seemed to me that this form and structure was exactly such as would result from the manner of aggregation which I had supposed. When a particle which ultimately forms the vertex of the cone starts on its downward career and encounters other particles, these adhere to its lower face. The mass, therefore, grows in thickness downwards; and as some of the particles strike the face so close to the edge that they overhang, the lower face continually grows broader, and a conical form is given to the mass above.

When a particle first starts, it moves slowly, and the force with which it meets the other particles is slight, and consequently its texture is loose; but as it increases in size and velocity, it strikes the particles which it overtakes with greater force, and so drives them into a more compact mass.

Assuming that the temperature at which hailstones are

formed is not greatly below 32°, the particles must actually freeze together. For the effect of squeezing two pieces of ice together at or near the temperature of 32° is to cause them to thaw at those points where the pressure is greatest, at which points they freeze again as soon as the pressure is removed.

In illustration of the force with which the particles strike the face of the hailstone, I instanced the action of the particles of sand in Mr. Tilghman's sand-blast used for cutting glass and other hard materials.

I also reverted to the possibility of making

Artificial Hailstones,

by blowing a stream of frozen fog against a small object, making, as it were, the cloud to rise up and meet the stone instead of the stone falling through the cloud.

I had not, however, then overcome the difficulty of obtaining such a stream of frozen fog; but I gave two sketches of plaster stones, which, as far as their shape and the striated appearance of their surface were concerned, closely resembled hailstones, and which plaster stones had been obtained by blowing some finely-divided plaster of Paris against small splinters of wood by means of a jet of steam.

In the discussion which followed my paper Dr. Crompton suggested

The Ether Spray,

such as is used in surgery, as a means of obtaining a frozen fog. And shortly after the Meeting I tried this ether spray, using an instrument such as surgeons use. But although I found that the spray would freeze any thing such as a small tube of water, I could get no deposit of ice particles on the outside of any object. I varied the form of the apparatus,

but with no better success; and for the time I abandoned the attempt.

What the cause of this failure was I do not precisely know; but I attribute it to some excess of alcohol in the ether then used, which was not methylated ether. That this might have been the cause occurred to me about two months ago. I then determined to try again, and combine a spray of water with that of ether. I now obtained the lightest ether which Messrs. Mottershead & Co. could supply. The specific gravity of this was ·717; and it was made from methylated spirit.

With this, somewhat to my surprise, I at once obtained a deposit of ice even without the water spray, and with the same apparatus I had previously used; it was not, however, until I used the combined spray of water and ether that I obtained any thing resembling a hailstone in appearance. But the first time I used this combination I obtained a small but well-shaped hailstone on the end of a match which I held pointed towards the spray.

The next time I tried, however, on another day, I did not succeed so well with the water as without it: when using the water-spray the deposit of ice was wet or half melted, while without the water I obtained a hailstone in much the same manner as I had obtained before with the water.

This difference in the results on the two occasions was at once explained by the different states of the air; for on the first occasion it had been cold and dry, whereas on the second it was warm and saturated. With the dry air the ether spray reduced the temperature so far below 32° that the particles of ice did not freeze together; the force of impact was not sufficient to cause them to thaw in the first instance; and hence the water spray was necessary to keep this temperature from falling too low; whereas with the

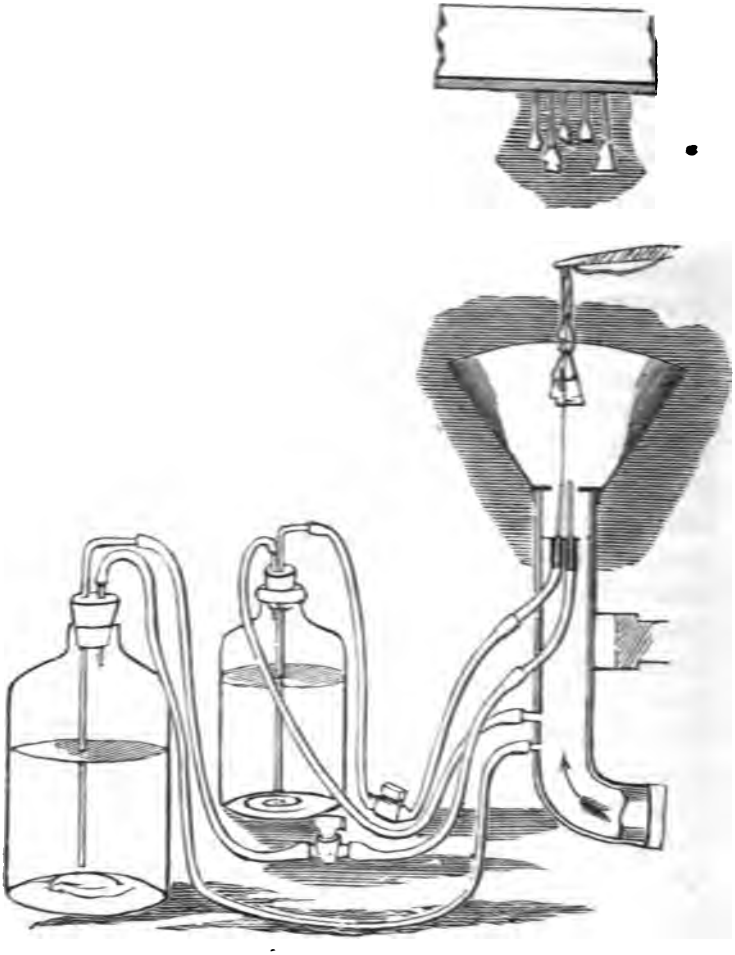
warm saturated air the ether did not reduce the temperature of the air and the vapour it contained much below 32°, and consequently, when the water spray was added, the water was only partially frozen.

I subsequently improved the apparatus so as to be able to regulate the supply of water and ether to the condition of the air.

The Apparatus.

This is shown in the accompanying sketch (p. 166). It consists of a brass tube half an inch in diameter, one end of which is connected with bellows capable of maintaining a constant pressure of about eighteen inches of water; on the other end of the tube is a cap, over the end of which is a flat plate or diaphragm having a central opening an eighth of an inch in diameter, which forms the aperture for the blast. Entering through the sides of the main brass tube are two small brass tubes which reach to within half an inch of the plate, and into the ends of which are sealed fine glass capillary tubes, the glass being very thin; these protrude just through the middle of the aperture, the one about one sixteenth of an inch and the other one thirty-second. Through these tubes the water and ether are separately introduced into the blast to form the spray; and it is mainly on the adjustment of these tubes that the efficiency of the apparatus depends. It is essential that the ether-tube should be slightly the longest; otherwise the ends become stopped with ice; and I find it better that the ether-tube should be somewhat larger than the water-tube. The bore of the tubes must be very small: but this is not sufficient; for unless the glass is very thin the spray will not be finely divided. Both the ether and water are forced through the tubes from bottles by connecting the interiors of these bottles with the bellows; and the quantities of ether and

water are regulated either by raising or lowering the bottles or by means of the cocks in the pipes.



The tube is fixed in an ordinary retort-stand so that the blast is vertical. If, then, a small splinter of wood is held downwards pointing into the spray, a lump of ice forms on the end of the splinter; and this lump has all the appearance

of the hailstone. It is quite white and opaque; it is conical in form, and has a rounded base and striated surface.

In this way I have formed stones from half to three quarters of an inch in diameter. When, however, the stones are growing large, it is necessary to move this splinter so as to expose in succession all parts of the face of the stone to the more direct action of the spray.

When using this apparatus in a warm room, I have found it best to fix a pad of blotting-paper over the jet at a height of ten or twelve inches. The surface of this pad is cooled by the spray and prevents radiation from the ceiling, which otherwise tends to melt the top of the stone. For a similar reason I have found it well to surround the blast with a wide cylinder or inverted cone of paper, which keeps off radiation without interfering with the action of the jet.

By sticking several splinters of wood pointing downwards into the pad, a number of stones may be made at once.

In the accompanying sketch are shown a medium-sized

stone, as well as one of the largest stones, attached to the splinters of wood. The surface of the cone where conti-

nuous is truly conical, or rather pyramidal; but this surface is broken, as it were, by steps; and a very marked fact is that all the continuous surfaces have the same vertex; and hence the different conical surfaces to which they belong have not the same vertical angle, the surface being exactly such as would be acquired by the fragments of a sphere so constituted that the fracture tended to follow radial lines.

Owing to the radiation of the surfaces from a common vertex and the steps which occur between the vertex and the base, the angle of the conical surface of the stone is greater near the vertex than near the base. Thus the smaller stones appear less elongated than those which are larger.

The fact that in the sketches of actual stones, which I gave in my last paper, I showed the steps as less pronounced and the angles as larger than they are in the artificial stones, is probably owing in some measure to my having formed my ideas from the observation of favourable specimens chosen from amongst those which fell. The larger angles were probably also, in part, owing to the smaller size of the actual hailstones, which were not much more than one fourth of an inch across. But I think that it is important to notice that the somewhat imperfect way in which the outside layers in the surface of the artificial stones are continued may be owing to the narrowness of the jet of air, which, on striking the stone, tends to diverge laterally rather than to flow upwards past the sides of the stone, as it would do if the jet were broader, or as the air must do when the stone is falling through it.

The rate at which stones can be formed depends on the amount of water which can be introduced into the spray, the larger stones taking from one to two minutes. At first sight this may seem to be somewhat slow; but the fol-

lowing estimate tends to show that the artificial are probably formed more quickly than the actual stones.

The speed of the jet of air at the point at which the stones are formed is nearly equal to that at which the larger stones would fall through the air. This is shown by the fact that if a large stone becomes accidentally detached from its splinter of wood it rather falls than rises, but when this happens with smaller stones they are driven up by the force of the blast.

I find that the speed of the blast varies from 150 to 200 feet per second, *i. e.* from one to two miles a minute. The larger stones, therefore, traverse from one to three miles of frozen spray. So that if we imagine a cloud as dense as the spray, it would have to be from one to three miles thick in order that the stones might, in falling through it, attain the size of the artificial stones; and considering that the stones would only gradually acquire a speed equal to that of the blast, the time occupied in falling through the cloud would, in all probability, be very considerable, at least from five to ten minutes, after the stone had acquired a sensible size.

As regards the proportion which the density of spray bears to that of a cloud, a comparison may be made from the fact that when working in saturated air at a temperature of 60° or 70° F., the condensation of vapour supplied sufficient ice to form the spray; and since it is probable that the dense summer clouds, from which hail is formed, result from the cooling of air from temperatures nearly, if not quite, equal to this, there is probably no great difference in the density of the clouds and the spray.

Snow Crystals.

I have not yet had an opportunity of examining the texture of these artificial stones under the microscope; but to

all appearance they consist of an aggregation of small spherical particles of ice; and it seems worthy of notice that, while nothing like a snow crystal appears ever to be produced in the ether spray, the moment the blast is stopped the end of the ether tube becomes covered with ice, which often assumes the form of snow crystals.

This appears to indicate the character of the difference between those conditions which result in snow and those which result in hail.

When the cloud-particles are formed at or above the temperature of 32°, and then freeze, owing to cooling by expansion or otherwise, the particles as they freeze retain their spherical form. This is what happens in the spray.

On the other hand, when saturated air at a temperature below 32° is still further cooled, the deposition of the vapour will be upon ice, and will take the form of snow crystals.

The aggregation of the snow crystals into flakes is, as I pointed out in my previous paper, accounted for by the larger crystals overtaking the smaller crystals in their descent, and the still more rapid descent of the flakes as they increase in size.

As regards the formation of raindrops, I have nothing to add to what was contained in my last paper. The same explanation obviously applies to both hail and rain; and any doubt which may have been left by the less direct arguments in my former paper will, I venture to think, have been removed by the verification of my predictions in the production of artificial hailstones so closely resembling in all particulars those formed by nature. In conclusion, I would thank Dr. Crompton for the suggestion of the means by which I have been able to produce these stones.

XXII. *On the Origin of some Ores of Copper.*—Part I.

By CHARLES A. BURGHARDT, Ph.D., The Owens College.

Read November 27th, 1877.

To endeavour to trace the various ores of copper to one fundamental source, and to study the relationships probably existing between the copper compound and its accompanying minerals, has appeared to me a subject of great interest; in fact it has engaged my attention for some time. The commonest occurring ore of copper is undoubtedly cuprite (Cu_2O), either independently or more often associated with malachite, azurite, brown iron-ore, and native copper. Scarcely a specimen of copper-ore can be obtained (with the exception of the sulphides) which does not exhibit more or less cuprite in intimate intermixture with the other ore; whence it must be inferred that all, or almost all the ores of copper have been formed by aqueous and not by plutonic action, as cuprous oxide is undoubtedly prepared with greater facility by the wet method (that is, from cuprous or cupric solutions) at a comparatively low temperature than by the dry method at a very high temperature. I will briefly state in which way cuprite (that is, the crystallized cuprous oxide) has been obtained by different authors, first by the dry method, and secondly by the wet method.

PREPARATION OF CUPRITE.

Dry Methods of Preparation of Cuprite.

1. A mixture of 24 parts of cupric sulphate and 29 parts

of copper turnings, heated to redness in a well-closed crucible, furnishes eventually a reddish-brown crystalline mass, which is cuprite.

2. A mixture of 100 parts of cupric sulphate and 57 parts of soda crystals is heated until all the water of crystallization is driven out; the residue powdered finely, 37 parts of copper turnings are added, and the whole mixture heated in a crucible to a white heat for half an hour. On washing out the cooled mass with water, cuprite of a very fine colour is obtained. (Wöhler and Liebig, Pogg. Ann. xxi. 581.)

3. Crystals of this substance are often observed in the slags obtained in the smelting of copper-ores.

It will readily be observed from the above that an intense heat is requisite for the formation of cuprite by the dry method.

Wet Methods of Preparation of Cuprite.

1. A solution of equal parts of cupric sulphate and grape-sugar is mixed with sufficient caustic soda solution to dissolve all the resulting precipitate, and then gently heated; cuprous oxide then separates out in the form of a crystalline powder. (Mitscherlich, Journal für practische Chemie, xix. 430.)

2. Bequerel (Compt. Rend. xliv. 308) describes a beautiful method which is applicable for the preparation of many minerals. He filled a test-tube with a neutral solution of cupric nitrate, placing a little cupric oxide and a clean strip of copper plate at the bottom, closed the tube air-tight and left it to itself for many months. In this way he succeeded in obtaining small shining cubes of cuprite.

3. A. Knop (Jahrb. f. Min. 1861, 508) states that if a mixture of solutions of cupric sulphate and ferrous sulphate be treated with an alkaline carbonate, carbonic acid gas is

evolved and a mixture of ferric hydrate and hydrated cuprous oxide is precipitated, the latter becoming crystalline after the expiration of some time.

Of the three methods given above, that of Knop undoubtedly furnishes one of the most probable processes by which many large deposits of the so-called tile-ore and copper-pitchblende have arisen; but there are again many other deposits of cuprite which may easily have been formed in other ways. I will not discuss at the present time the various opinions upon the formation of copper-ores expressed by some very eminent and competent authorities; but I may mention that Dr. Ferdinand Wiebel, in an exhaustive treatise ('Das Gediegen-Kupfer und Roth-Kupfererz,' Hamburg, 1864), concludes that the native metal and cuprite have in *all* cases been formed from a cupric sulphate solution (obtained by the oxidation of copper-pyrites) through its reduction by a solution of ferrous sulphate (also derived from the oxidation of copper-pyrites). This statement is doubtless correct in the case of some formations; but there are many others where it is questionable. Gustav Bischoff points out, in his 'Chemical and Physical Geology,' that "It is certain that the contents of a lode are of later date than the adjoining rock; so that it can be shown that these contents originate from the adjoining rock. If, then, it can be ascertained what compounds of the metal exist in that rock, the previously existing minerals may be distinguished from those of later date; but the determination of this point is attended with great difficulties, and is generally impossible." It is a well-ascertained fact that all the ores of copper are found mostly in crystalline rocks or metamorphic rocks derived from them (some ores, however, being found in newer formations); and Struve ('Ueber die Nachbildung der natürlichen Heilquellen,' Heft ii. 17) has proved in the most conclusive

manner that granite, porphyry, phonolite, gneiss, basalt, clay-slate, trachyte, &c. contained a more or less appreciable amount of sodium chloride; and it is also well known that nearly every spring-water flowing from rocks contains sodium chloride or a chloride of one of the alkaline earths (magnesium chloride). Further, it has been shown by Delesse (Jahrb. f. Min. 1862, 605) that all rocks are *saturated* with water; and it is also a well-ascertained fact that iron-pyrites (FeS_2) is disseminated throughout *all* crystalline rocks. Having these facts before me, I made an experiment in order to ascertain what reactions would occur on heating iron-pyrites and a solution of cupric chloride together in a sealed tube at a moderately high temperature. Small pieces of pure iron-pyrites were placed in a glass tube, then covered with a moderately strong solution of pure cupric chloride, and the tube sealed up and heated for 14 days at a temperature varying from 135° to 210° C.; but the greater part of the time the temperature oscillated between 150° and 170°. On the seventh day the colour of the cupric chloride had become considerably lighter, and a small deposit of violet-red crystalline cuprite was observed adhering to the sides of the glass tube, whilst on allowing the tube to cool it was observed that a very considerable amount of cuprous chloride had crystallized out in beautiful transparent colourless tetrahedrons. On the fourteenth day the tube was opened (as no further changes had taken place), and the contents examined, and found to consist principally of cuprous chloride with a small quantity of unaltered cupric chloride, there being at the same time a good amount of ferric sulphate and cupric sulphate present. The deposit on the glass tube was cuprite. I was unable at the time to make any quantitative determination of the various substances formed in this reaction, and am therefore not prepared to say precisely what the reaction was.

Of course the iron-pyrites was considerably attacked; but there was no separation of free sulphur. Free hydrochloric acid was also present, but not in sufficient quantity to dissolve the small quantity of cuprite formed on the tube. The insoluble crystallized cuprous chloride was carefully washed in distilled water in order to free it from the substances already mentioned, then placed in another glass tube, covered with distilled water, and the tube sealed up and heated for seven days in an air-bath at temperatures varying from 160° to 180°. On the second day a very marked amount of a bright-red substance had already formed on the sides of the tube, and here and there minute green spots of a substance somewhat resembling atacamite ($CuCl_2,3CuHO+xH_2O$) in colour. This tube was heated at the temperatures given for about two weeks, when it was observed that although the red deposit did not increase in quantity, the cuprous chloride was slowly undergoing decomposition, a black powder having separated out. On opening the tube a smell of hydrochloric acid was perceived. The liquid portion in the tube was poured out, and the solid portion (which principally consisted of unaltered cuprous chloride crystals with a little of the dark-coloured powder) well washed with water. By levigation it was easy to separate the powder from the crystals and to submit it to a further examination. In order to free it completely from cuprous chloride, it was heated in a beaker with a solution of ammonium sulphate in water, which dissolved out all the cuprous chloride and left the powder intact; and the latter, on being dissolved in nitric acid, did not give any reaction for chlorine, whilst the presence of copper was very evident; hence there is scarcely any doubt about this substance being cupric oxide. The liquid portion contained free hydrochloric acid and cupric chloride.

When the red deposit first commenced to form upon the

sides of the tube, a small quantity of violet-red crystalline cuprite was undoubtedly also present, but disappeared on the experiment being continued. I dissolved the red deposit in nitric acid, and detected the presence of a large quantity of chlorine in the solution, also that of copper. Therefore I conclude that this red substance is the so-called cuprous oxychloride described by Wöhler (Ann. Chem. Pharm. cxxx. 376). On exposing the substance to the air, it takes up oxygen and acquires a beautiful apple-green colour, becoming a cupric oxychloride which has probably the same composition as atacamite.

Chalcotrichite.—Being desirous of ascertaining what the action of a moderately strong solution of sodium chloride in water would be upon pure artificially prepared cuprous oxide when heated with it in a sealed tube, I employed the following method, viz.:—The tube was heated in an air-bath for five days at a temperature varying from 150°–180°, when small traces of a green substance (probably atacamite) were observed, the cuprous oxide had become dark brown in colour, and on the sides of the tube there were small beautiful radiating tufts of an orange-red-coloured substance closely resembling the prismatic variety of cuprite called chalcotrichite. Continuing to heat the tube, on the eighteenth day it was opened, when it was found that no further formation of the prismatic substance had taken place, but there was a beautiful deposit of highly crystalline violet-red-coloured cuprite on the sides of the tube. The fibrous substance was dissolved in nitric acid (after having been boiled in distilled water for several hours and carefully washed) and tested for chlorine, which was found to be entirely absent, whilst copper was present in large amount: hence there can be no doubt that this prismatic substance was the rare mineral chalcotrichite. From this experiment it is evident that cuprous chloride was formed at first by

the reaction between the sodium chloride and cuprous oxide, and this cuprous chloride was decomposed into cuprous oxide again and deposited on the sides of the tube as above described. I shall refer to the importance of the occurrence of chalcotrichite in conjunction with cuprite when I come to consider the origin of all the copper-ores in a future paper, and will therefore describe another series of experiments, having for their object the direct formation of malachite.

Malachite.—Great difficulty was experienced in carrying out these experiments for any length of time, owing to the frequent bursting of the tubes, due, no doubt, to their being somewhat weakened by the formation of silicates upon the surface of the glass. Pure artificially prepared cuprous oxide was placed in a glass tube, and covered with distilled water saturated with perfectly pure carbonic acid gas, and then sealed up and heated in the air-bath at a temperature varying from 150° to 175°. After heating about fifteen days, the tube was opened and allowed to stand exposed to the air, when a thin film of malachite was observed coating the small clumps of suboxide here and there. The action of oxygen was found to be absolutely essential in the production of malachite by this method, there being no sign of any such formation in the tube before it was opened. Thus there can be no doubt that the greater part of the malachite formations have been produced by the action of water containg carbonic acid gas in solution upon cuprous oxide, as the malachite of the Gumeschewskoi Mine, near Ekaterinenburg, in Russia, is found deposited in a dark-coloured clay which penetrates a small limestone ridge, the whole formation finally resting upon chloritic slate. I have examined numerous specimens of malachite from all parts of the world, and have always observed strong evidence of the action of water upon them, there being numerous

cavities always present, which exhibit deposits of cuprous oxide or hydrated ferric oxide in their interiors, and occasionally amorphous silica and calcite. Gustav Rose (Mineralogisch-geognostische Reise nach dem Ural, dem Altai und dem kaspischen Meere, 1837) was of opinion, after examining the copper formations in the Ural and Altai districts, that the cuprite there was formed by direct oxidation of the metal, and, secondly, that the large malachite deposits were formed from the cuprite. This opinion probably well expresses the actual process which eventually resulted in the formation of malachite, as the presence of the clay surrounding the ore, the limestone formation, and the hydrated ferric oxide and hornstone (amorphous silica) shows that the former neighbouring crystalline rocks must have been submitted to the powerful action of water, which, in all probability, contained carbonic acid gas. Rose does not give (so far as I am aware) any experimental proofs of his theory of the formation of the great copper deposits of the districts above mentioned. I considered it therefore necessary to endeavour to determine experimentally the accuracy of his statement. For this purpose I placed perfectly clean polished strips of chemically pure copper sheet in a glass tube, and covered them with distilled water saturated with carbonic acid gas; the tube was then sealed up and heated in a water-bath at 100° for some time. In three days I observed a decided formation of a film of cuprous oxide (which was crystalline) upon the metal; on the seventh day there were small particles here and there of a green substance, which was undoubtedly a basic copper carbonate (very probably malachite). On continuing the heating of this tube for a few days longer at the same temperature, a considerable amount of cupric oxide was formed in scales, arising, doubtless, from the decomposition of the malachite, as it is a well-known fact that malachite, on

being boiled in water, decomposes into cupric oxide, carbonic acid gas, and water. In order to see what effect pure distilled water would have upon metallic copper under similar circumstances, I placed a large strip of clean polished sheet-copper in a tube, covered it with distilled water, and sealed the tube up, heating it in the water-bath as in the previous experiment. I was astonished to find on the third day that the copper was strongly coated with *cupric* oxide; so that it is evident that pure water has a greater chemical action as an oxidizing agent than water containing carbonic acid gas. Having thus obtained the results described, I may be allowed to draw some conclusions from them, as regards the primary copper-ore and the eventual formation of the other ores from it, briefly as follows, viz.:—

1. In all probability the crystalline rocks contain disseminated throughout their mass extremely minute quantities of metallic copper (*when perfectly fresh*), Bischoff mentioning many rocks in which small quantities of cupric oxide have been detected where its presence would never have been expected.

2. By the action of water, or a solution of carbonic acid gas in water, the metallic copper particles were converted *in situ* into one of the three following substances, viz. cuprous oxide, cupric oxide, and malachite.

3. By the action of water partially charged with sodium chloride (derived from the surrounding rock-masses), cuprous chloride and cupric chloride were formed and carried down into the fissures below, where, by the action of a heat which need not have exceeded 160°, the *cuprous chloride* (for the cupric chloride would be completely reduced to cuprous chloride by the action of the iron-pyrites which is universally present in all crystalline and metamorphic rocks) was decomposed into cuprite and cupric

oxide and deposited in the fissures or lodes. The ferric sulphate simultaneously formed in the reduction of the cupric chloride would eventually decompose into hydrated ferric acid and accompany the cuprite in the lodes, whilst the free hydrochloric acid formed in this process would readily attack the neighbouring rocks, thus causing the deposition of gelatinous silica in the lodes.

It will now be my next endeavour to study the chemical composition of all the rocks occurring in the immediate neighbourhood of copper-mines, and particularly the evidences of decomposition of these rocks at the place of contact with the metalliferous lode, in order to ascertain whether the above conclusions are correct. In a future communication I hope to deal with the statement, made by great authorities on this subject, that the sulphides of copper are the primary and oldest compounds of that metal.

XXIII. *On the Construction of a Room or Series of Rooms free from Germ Life, proposed for use in the performance of Surgical Operations.* By WILLIAM THOMSON, F.R.S.E.

Read November 27th, 1877.

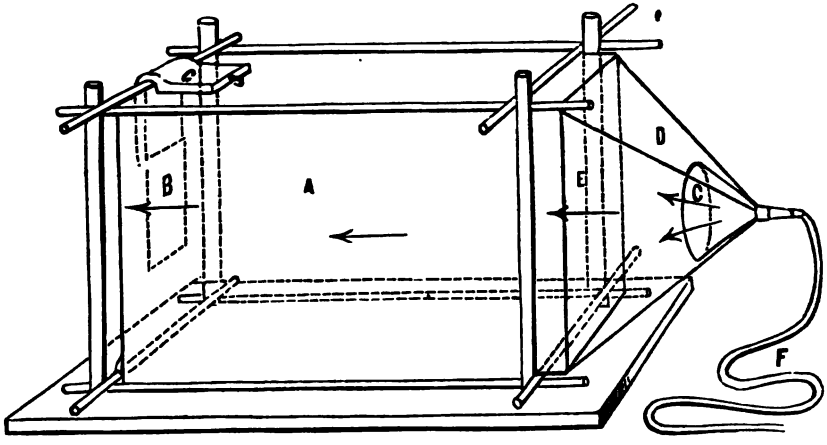
MY object in bringing this paper before the Society is to show what I consider to be a valuable application of a well-known principle, viz. that of filtering from the

ordinary air the innumerable spores which are constantly found floating about in it, whereby in a room or series of rooms the air may be rendered optically pure. The practicability of producing and afterwards retaining in a room or rooms of ordinary dimensions an optically pure atmosphere was suggested to me by the results of a large number of experiments which I have carried on during the last seven or eight years, but more especially those obtained from a series of experiments which I commenced about the beginning of this year, on the cultivation of different fungi, the spores of most of which are found in large numbers floating about in our atmosphere.

Flour paste suggested itself as the best soil I could obtain on which to grow these fungi. When flour paste is exposed to the air for a few days, as is well known, fungus makes its appearance, and soon completely covers its surface; and if a number of flour pastes be made and exposed in different places, it will be found that different kinds of fungi spring up in each, showing a splendid variety of different colours and shades of the same colour. It was with a view to isolate and study these fungi that one of my last series of experiments was commenced; and it became necessary to look for a means of sowing the seeds of each fungus on a soil which should originally be free from spore-life, and afterwards should be effectually protected from the falling of other spores from the air onto it, and, further, should, if possible, give a free supply of air to the microscopic plants.

After a few experiments, I found that this could be done effectually by placing flour paste in an ordinary glass shade turned upside down, covering the mouth of the shade with a thin layer of cotton wool placed between two pieces of cloth, the three layers being fixed round the edges of the glass by an elastic band. The paste, when boiled in the

vessel thus closed, was afterwards found to remain free from fungus-life for any length of time. Having thus been successful by such a rough arrangement in preventing the entrance of fungus-spores, I next decided to try the same experiment on a rougher and larger scale, and consequently had the rough model made which I show here this evening [see figure].



Each side of this box or room is made of thin layers of cotton wool sewn between two pieces of cloth, with a glass (B) in front, which serves equally as door and window. A conical-shaped arrangement (D) made of stout paper is pasted to a square frame at its base, which fits against one end of the room. The stem of an ordinary glass funnel (*c*) is passed through the apex of this paper cone, and there fixed by means of a piece of string. An india-rubber tube (F), communicating with a pair of bellows, is put on the end of the stem; the large end of the funnel is covered with a thin layer of cotton wool placed between two pieces of cloth and fixed round the funnel-edge by means of an elastic band. When the window at the other end of the room is opened

and the bellows worked, a current of air passes through the layer of cotton wool and two pieces of cloth on the mouth of the funnel, then through the end of the room (E) (which also acts as a filter), and, moving bodily along the room, escapes at the window, little or no special currents or draughts being apparently produced. This I observed by previously filling the room with fumes. The force of the air, by passing through the two layers of cotton wool, was evidently equally distributed throughout the whole space of the room. My first experiment with this arrangement was made by simply working the bellows for about twenty minutes to endeavour to drive the impure air with its floating particles and spores out of the room by passing into it the filtered air from the bellows, then boiling one half of some flour paste in a basin inside the room for a minute or two, and, lastly, closing the window with the glass, the other half of the paste being boiled in a similar basin and left outside exposed to the ordinary or contaminated air. After a few days the difference between the two pastes was very marked; the surface of the one left outside the room was completely covered with fungus, whereas only four or five little specks of fungus were observed to be growing on the surface of the one inside the house, which soon developed into four or five little buttons of fungus. I considered that the spores which had fallen on this flour paste must have been sticking to the sides and roof of the room; and to eliminate this assumed cause of error I drew out the supporting rods of the room, crushed it up and boiled it in water for half an hour, then allowed it to dry, and repeated the same experiments with the two basins of flour paste, the bellows being first worked, and the glass then taken out and laid down in the ordinary or contaminated air, then wiped with a clean cloth and replaced. In a few days afterwards the paste outside the house was completely covered with fungi,

whilst the one inside was absolutely free from it, and it remained free for nearly a month, when one little speck was observed on it, which soon developed into a little button of fungus. This showed that for three weeks after the paste was placed in the room no spore had fallen on it. The only cause of failure I could see was the glass, upon which some spores must have adhered; consequently I had a bag (C) made and sewn on above the window, into which the glass could be pulled in opening the window. When this was done, the house was again crushed up and boiled in water, again placed in position, the paste boiled, the window closed; and in this third trial the experiment was perfect. No fungus was developed on the paste inside the house; and after some months it dried up to a hard mass. Thus these thin sheets of cotton wool forming a room of 10,032 cubic inches capacity, *i. e.* capable of holding 36 gallons of air, were sufficient to prevent the entrance of spores into it, and thus to preserve the flour paste from decomposition during several months, till it had completely dried up.

These results, I think, conclusively show that ordinary large rooms may be constructed, and ventilated with filtered air by means of fans, so that flour paste, taken as a test standard, would remain in them free from fungus-life; and I believe that such a room or series of rooms might be of great advantage in surgery—perhaps as a means of preventing spores from entering the wounds of patients, and so doing away with or lessening the onus put upon the antiseptic treatment, or in giving a better chance of success in serious surgical operations. It is believed by many medical men that pus and blood-poisoning may be generated in a wound from the blood-corpuscles or bioplasts in the body, and that such developments have nothing to do with the germs of the outside air, and that

the antiseptic treatment is employed to arrest their development. But no one, as far as I know, has ever proved this beyond doubt; and such an arrangement of a room or rooms as I suggest would, I believe, be one mode of experimenting, whereby the whole or a great part of a serious obstacle to such researches might be eliminated. But, in passing, I may mention one slight incident in reference to the development of pus in wounds, which would tend to show that certain germs floating in the air are capable either directly or indirectly of developing pus. Some years ago, when working with a solution of albumen which had been exposed to the air for some time, and which was then putrid and swarming with bacteria, I accidentally cut my finger, and some of the putrid liquid entered the wound. I washed it out as well as possible, and thought nothing more of it. Next morning it was slightly swollen and had gathered. I opened it with a knife and microscopically examined the matter, expecting to find bacteria; but, to my surprise, no bacteria could be observed, and all that could be seen were simply pus-cells.

The arrangements which I should suggest on the large scale are:—a long room or series of rooms in a line, at one end of which should be fitted a fan behind a good filter of cotton wool; a long pipe with a series of Bunsen burners set along it, so that, when all were lit, a line or sheet of flame would be produced which might be slowly passed along and made to play on every part of the floor, walls, and roof of the room, beginning near the end at which the fan is working, and going gradually towards the door; by this means any spores adhering to the walls would be destroyed, and no air could pass back to pollute the walls or floor which had been thus purified. A stove might be arranged at the door end of the room, by which cotton garments to cover the ordinary clothes of the surgeons and

attendants might be heated to a temperature presumably sufficient to destroy or paralyze the vitality of any spores which might be adhering to them, and where the knives and other appliances used might be previously heated, and where water to be used in washing the wounds might be heated under pressure. With such an arrangement at an hospital it seems to me that one interesting mode of investigation into some most important subjects might be commenced.

XXIV. *Note on Metallic Niobium and a new Niobium Chloride.* By Professor H. E. Roscoe, F.R.S. &c.

Read December 11th, 1877.

The history of the investigation of the two closely related metals Niobium and Tantalum is of interest as showing how, step by step, the difficulties and uncertainties which surround the examination of analogous and rare substances such as these are overcome; and I may therefore be allowed shortly to state the growth of our knowledge on this subject.

In the year 1801 Hatchett discovered a new metal in the mineral columbite from Massachusetts; and to this he gave the name of Columbium. Ekeberg, a year later, found a similar new metal in a rare Swedish mineral; and to it he gave the name Tantalum. Wollaston, in 1809, came to the conclusion that these two metals were identical. Berzelius next examined the metal contained in the Swedish tanta-

lite; and afterwards Wöhler found these metals in several other minerals. Heinrich Rose next investigated this subject; but, in spite of persistent effort, the results of his experiments left the matter in a still less satisfactory condition, inasmuch as he first came to the conclusion that two more new metals were contained in these minerals, to which he gave the names of Niobium and Pelopium, whilst at a later period he decided that both niobic and pelopic acids were different oxides of the same metal, for which he proposed the names niobic and hyponiobic acids.

Then, again, in 1860, v. Kobell thought that he had found a fourth new metal, which he called Dianium, in the same minerals; and Hermann believed that he had discovered in them a fifth metal, to which he gave the name of Ilmenium. It is to Marignac and Bloomstrand that we owe a deliverence from this state of contradiction and uncertainty. They independently proved that only two metals in reality exist, Niobium and Tantalum, and that these are present in varying proportion, in most of the minerals in question. They showed that the compounds derived from Rose's niobic oxide contain mixtures of niobic and tantalic oxides, whilst his hyponiobic oxide is pure niobic oxide.

Bloomstrand arrived at this result by a series of analyses of the chlorides. He proved that the white hyponiobic chloride of Rose is, in fact, an oxychloride, whilst the yellow niobic chloride contains no oxygen.

Marignac, on the other hand, came to a similar conclusion by an investigation of the fluorides of niobium and tantalum, and pointed out that the oxides are pentoxides, Nb_2O_5 and Ta_2O_5, whilst the niobium chloride and oxychloride are represented respectively by the formulæ $NbCl_5$ and $NbOCl_3$.

All the other supposed metals turned out to be either simple mixtures of these two in varying proportions, or

such mixtures containing in addition some other metal, such as tungsten, titanium, or iron.

Troost determined the vapour-densities of niobium pentachloride and oxychloride, obtaining the number 9·6 for the first and 7·87 for the second, agreeing closely with the calculated densities when the atomic weight of niobium is taken to be 94.

Some time ago I obtained several crystals of Greenland columbite, which possesses a specific gravity of 5·389, and which contains, as Marignac has shown, little or no tantalum. From this material I prepared niobium pentoxide (Nb_2O_5), niobium pentachloride ($NbCl_5$), and niobium oxychloride ($NbOCl_3$) by well-known methods.

I have likewise prepared metallic niobium and a new chloride of niobium. The results of these experiments are contained in the following communication.

The following results of analyses of the pentachloride and oxychloride serve to show the purity of the material:—

	Pentachloride.	
	Calculated.	Found.
Niobium	34·62	34·38
Chlorine	65·38	65·55
	100·00	99·93

	Oxychloride.	
	Calculated.	Found.
Niobium	43·42	42·93
Chlorine	49·19	49·34
Oxygen (diff.)	7·39	7·73
	100·00	100·00

Metallic Niobium is difficult to prepare; and precautions similar to those employed for the preparation of metallic vanadium must be taken. It is obtained as a bright steel-grey, shining, metallic crust by first passing the vapour of the pentachloride mixed with hydrogen repeatedly through a red-hot glass tube. The metallic crust then deposited on the side of the tube is next strongly ignited in a porcelain tube in a current of hydrogen until no further evolution of hydrochloric acid occurs.

In order to avoid as completely as possible access of

traces of oxygen, the very hygroscopic pentachloride was prepared in a narrow tube, and this was transferred bodily into the wider tube in which the preliminary heating with hydrogen takes place.

The metal thus obtained was analyzed by placing a weighed quantity in a porcelain boat and heating it in a glass tube in a current of air. The metal takes fire and burns brightly to the pentoxide, a small trace of oxychloride being usually deposited, existing as impurity in the metal. In one instance, when the metal was allowed to oxidize very slowly, the formation of an indigo-blue-coloured lower oxide was first observed; this afterwards passed into the yellow pentoxide on further heating.

The following analyses were made with metal obtained in two distinct operations :—

	gram.
I. Weight of metallic niobium taken	0·3963
„ niobium pentoxide obtained ...	0·5430

This corresponds to 0·3810 of metal; or the substance contains 96·14 per cent. of niobium.

	gram.
II. Weight of metallic niobium taken	0·3900
„ niobium pentoxide obtained ...	0·5403

This corresponds to 0·3790 of metal; or the substance contains 97·18 per cent. of niobium.

That the substance thus prepared is not, as has been supposed, a hydride, is shown by the fact that the second of the above specimens on analysis was found to contain only 0·27 per cent. of hydrogen.

Metallic niobium possesses a steel-grey colour and bright metallic lustre. It is remarkable that it is not attacked by hydrochloric or nitric acids, either dilute or concentrated, and *aqua regia* acts only very slightly upon it. Strong sul-

phuric acid, on the other hand, dissolves the metal quickly, yielding a colourless solution. The metal does not take fire in chlorine gas at the ordinary temperature; but it does so when gently heated, with formation of pentachloride and a trace of oxychloride, owing to the unavoidable presence of a small quantity of oxygen in the metal experimented with.

The specific gravity of metallic niobium at 15°·5 is 7·06, as a mean of two well-agreeing determinations made with half a gram of substance.

Niobium Trichloride, $NbCl_3$.—When the vapour of niobium pentachloride is passed through a heated glass tube it is slowly decomposed, a black deposit of trichloride being formed on the sides of the tube. Niobium trichloride is generally obtained as a black crystalline crust which possesses an almost metallic lustre, and closely resembles in appearance a film of sublimed iodine. It is sometimes deposited in long needle-shaped crystals which exhibit a dichroic lustre. The trichloride is non-volatile; it is not deliquescent, nor is it decomposed by water or ammonia; but when brought into contact with dilute nitric acid it is at once converted into niobic and hydrochloric acids. Heated in the air it emits dense white fumes.

For the purpose of analysis the trichloride, from several different preparations, was in some cases decomposed by nitric acid directly, and in others first fused with sodium carbonate and the fused mass treated with nitric acid.

	No. 1.	No. 2.	No. 3.
Substance taken	0·2781	0·4406	0·5344
Nb_2O_5 obtained	0·1930	0·3000	0·3611
AgCl	0·5681	0·9333	1·1218

	Calculated.	No. 1.	No. 2.	No. 3.
Niobium (Nb)	46·88	46·68	47·76	47·42
Chlorine (Cl_3)	53·12	52·53	52·40	51·93
	100·00	99·21	100·16	99·35

When the vapour of niobium oxychloride is passed over red-hot charcoal in an atmosphere of carbon dioxide, no change occurs; and the pentachloride is obtained when the carbon dioxide is replaced by chlorine.

A remarkable reaction occurs when niobium trichloride is heated in a current of carbon dioxide; this gas is decomposed, a white sublimate, consisting of oxychloride, is obtained, and the corresponding quantity of carbon monoxide is formed.

That this is the case was ascertained by analyzing the oxychloride and by collecting the gas over caustic potash and measuring the volume of insoluble gas which came over. This gas was shown to burn with a blue flame; and the quantity was found in two experiments to correspond closely with the theoretical amount. The following is the composition of the oxychloride thus prepared:—

	Found.
Niobium	43.48
Chlorine	47·03
Oxygen	9.49
	100·00

A decomposition of carbon dioxide by a metallic chloride has hitherto not been observed. Phosphorus trichloride absorbs free oxygen on distillation in the air, with formation of oxychloride; but it does not decompose carbon dioxide.

XXV. *On the Geometrical Representation of the Equation of the Second Degree.*

By CHARLES CHAMBERS, F.R.S., Superintendent of the Colaba Observatory, Bombay. Communicated by J. A. BENNION, F.R.A.S., A.C.P.S.

Read December 26th, 1877.

FOR every value from $-\infty$ to $+\infty$ of one of the variables in an equation of the second degree between two variables, there are corresponding pairs of values of the other variable; and for every value from $-\infty$ to $+\infty$ of the second variable there are corresponding pairs of values of the first variable. The corresponding pairs of values are of two classes, viz. first those which are not, and secondly those which are, affected by the symbol $\sqrt{-1}$. The Cartesian method gives a perfectly clear geometrical representation of that part of the equation for which the values of both the variables are real, but discards as unintelligible that part for which either of those values is imaginary. In the simplest case (of rectangular coordinates) the unit adopted in the Cartesian system, as the analogue of the unit of the algebraical equation, is a unit of length of a mathematical straight line, with a convention as to signs which we need not describe. For a reason that will presently appear, we here make the otherwise simple observation that, if we substitute (in the Cartesian method) the idea of pairs of coincident ordinates and take the product of their values instead of the square of a single ordinate, we only add a convention that leaves the analogy between the algebra and the geometry in question untouched. We further remark that this analogy will still be perfect, if we conceive

of the two coincident ordinates as tubes instead of mathematical straight lines, provided the tubes be of infinitesimal transverse section, which may be of circular form and, say, of radius R, and provided further that the numerical values of the tubes are the quantities of curved surface which they possess, the convention as to signs being unaltered. The unit of curved surface must obviously be $2\pi R^2$. Instead, however, of the curve being described by the motion of the central point of the extremity of the ordinate, let it be described by a diameter, through that point, perpendicular to the plane of the coordinates; it will still be of infinitesimal depth. Further, let us take as the fundamental ideal of the tubular ordinate the one whose height is R. It may be varied in value by elongation in a positive or negative direction, its diameter remaining constant; or it may be varied by dilatation into larger and larger circles, its height remaining constant: in the latter case the ordinate, which may be called circular, to distinguish it from the tubular ordinate, will be of positive sign if it be on the positive side of the coordinate plane, which is perpendicular to the tubular ordinate, and of negative sign if on the opposite side of that plane. Elongation to unit length of the tube, and dilatation to unit radius of the circle, alike make the quantity of curved surface $2\pi R^2$, which is the unit of this magnitude. Adopting this latter unit as the analogue of the algebraical unit, we shall thus have two distinct but connected systems of variation of the magnitude—that of elongation yielding tubular ordinates analogous to and coextensive with the ordinates of the Cartesian geometry, and that of dilatation yielding the circular ordinates, the nature of which we now proceed to consider.

Just as in our modified Cartesian system the ordinate is erected with the axis of the tube at the extremity of the corresponding coordinate, and with a length determined by

the equation, so here the circular ordinate must be described (on the plane perpendicular to the tubular ordinate) with its centre at the extremity of the corresponding coordinate, and its radius the function of the latter coordinate given by the equation. Also as the curve, in the Cartesian system, separates the region of greater ordinates than its own at any point from the region of lesser ordinates, so here the curve must be such as will do the same; in other words, it must be the envelope of the series of circular ordinates. The reason will now appear why we substituted the product of a pair of coincident tubular ordinates for the square of a single ordinate: one of the pair we suppose to be produced by the elongation of a fundamental tube of infinitesimal height and radius, on the positive side of the perpendicular coordinate plane, and the other by elongation of a like tube on the negative side of that plane. The equal dilatation of these tubes will produce, not coincident, but adjacent circular ordinates, and these of opposite sign, of which the product will therefore be a negative square. The curve will be described by the line of intersection of contiguous circular ordinates, that line being of infinitesimal length 2R, half on the positive side and half on the negative side of the plane of the circles.

Once again we introduce an arbitrary convention that is only additional to, not in contravention of, those of the Cartesian geometry. In whichever manner the pair of fundamental tubes enlarge, whether by elongation or dilatation, we regard the product of the pair of ordinates produced as the counterpart of the square of the corresponding variable in the algebraical equation. Thus, if γ be the magnitude, regardless of sign, of the geometrical ordinate, then, when $y^2=a^2$, we may for the algebraical y^2 substitute $(+\gamma)(+\gamma)$ or $(-\gamma)(-\gamma)$, and we obtain the result $\gamma=\pm a$; and when $y^2=-a^2$, we obtain $(+\gamma)(-\gamma)=-a^2$, whence

$\gamma = \pm a$. In this way we avoid altogether the introduction of the algebraical symbol $\sqrt{-1}$ as affecting the geometrical representation of the equation. In the case of circular ordinates it is obvious that two points in the curve—that is, in the envelope of a series of circles whose centres are on a straight line—have the same ordinate and abscissa, one point, namely, on each side of the straight line.

The writer is prepared to have what he has advanced so far, regarded as rather curious than instructive, unless it can be shown that the system he describes leads to interesting results. In what follows it will be shown that such results are not wanting.

We now write down a series of equations, and construct a figure to represent them; and we then proceed to comment on the equations and curves, and on their points of interest in connexion with what has gone before.

$$\frac{x^2}{a^2}+\frac{y^2}{b^2}=1 \quad . \quad . \quad . \quad (a) \qquad \frac{x^2}{a^2}+\frac{y^2}{b^2}=-1 \quad . \quad . \quad . \quad (a')$$

$$\frac{x^2}{a^2-b^2}-\frac{z^2}{b^2}=1 \quad . \quad . \quad (\beta) \qquad \frac{x^2}{a^2-b}-\frac{z^2}{b^2}=-1 \quad . \quad . \quad (\beta')$$

$$-\frac{y^2}{a^2-b^2}-\frac{z^2}{a^2}=1 \quad . \quad (\gamma) \qquad -\frac{y^2}{a^2-b^2}-\frac{z^2}{a^2}=-1 \quad . \quad (\gamma')$$

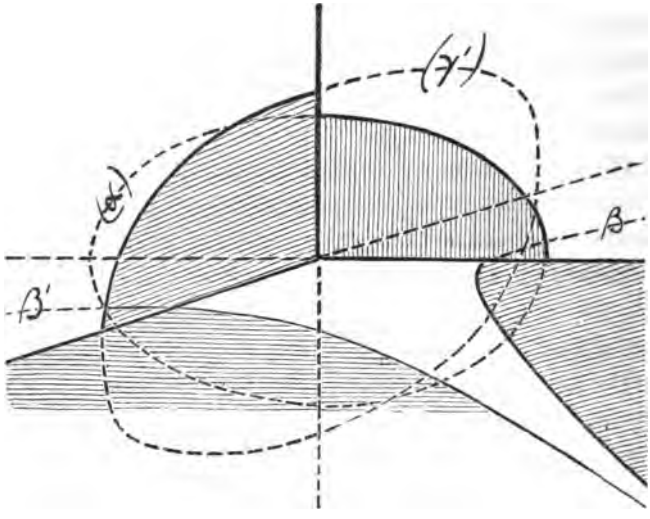
Of equation (a) we have the following cases, a^2 and b^2 being always positive numbers:—

Case 1. When x^2 and y^2 are both positive numbers. The ordinates are both tubular; and the equation is represented, as in the Cartesian system, by the ellipse (a), whose semi-axes are a and b.

Case 2. When y^2 alone is negative—that is, when x^2 is positive and greater than a^2. The x ordinates are tubular, and the y ordinates circular; and the equation is represented

by an envelope on the xz plane, the Cartesian equation to which we shall presently show to be (β), viz. that of the hyperbola β (fig. 1). This hyperbola has its vertices in the foci of the ellipse (α), and its foci, therefore, at the vertices of the ellipse; and its transverse axis is of the same magnitude as that of the ellipse.

Fig. 1.



Case 3. When x^2 alone is negative—that is, when y^2 is positive and greater than b^2. The y ordinates are tubular, and the x ordinates circular. The envelope of the latter has (γ) for its Cartesian equation, and is an impossible curve; or, in other words, there is no intersection of the circular ordinates, and therefore no envelope.

Case 4. When both x^2 and y^2 are negative. The x and y ordinates are both circular; and each pair being compounded (in the manner of the compounding of moments of rotation) forms a single circular ordinate, the plane of which is different from that of every other compound ordinate; hence

there can be no intersection of compound ordinates, and these can have no envelope.

The three equations (α), (β), (γ) possess this remarkable reciprocal property. If any one of them be taken as the equation to be represented, it is itself the Cartesian equation of the corresponding curve when x^2 and y^2 are both positive; and the two others are Cartesian equations of the respective envelopes when either x^2 or y^2 is alone negative; and thus the ellipse (α) and the hyperbola (β) are the representative curves of all the three equations.

The three similar equations (α'), (β'), (γ'), in which the only difference from the former three is in the reversed sign of unity on the right-hand sides, possess, of course, the same reciprocal property. But here the impossible curve is on the xy plane; that on the xz plane is the conjugate hyperbola to the former one, and is on the same plane with it, and has therefore the same asymptotes, whilst the curve on the yz plane is an ellipse whose semi-axes are a and $(a^2-b^2)^{\frac{1}{2}}$.

When $a=b$—that is, (α) is a circle—the three other real curves coincide with the z axis.

To find the Cartesian equation to the envelope of the circular ordinates in the case No. 2, in which y^2 alone is negative, we have, for the positive square of the type radius of the circles, $\frac{b^2}{a^2}(x^2-a^2)$, and, for the equation to a type circle,

$$(\xi-x)^2+\zeta^2=\frac{b^2}{a^2}(x^2-a^2), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where x is the variable parameter; hence, taking the first derivative,

$$-(\xi-x)=\frac{b^2}{a^2}x, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and eliminating x by means of (1) and (2), we obtain

$$\frac{\xi^2}{a^2-b^2}-\frac{\zeta^2}{b^2}=1, \qquad \text{or } \frac{x^2}{a^2-b^2}-\frac{z^2}{b^2}=1,$$

which is the equation (β) written above. Similarly, by dealing with the impossible ordinates to the y abscissæ, we obtain the equation (γ).

The same three equations (α), (β), (γ) have the reciprocal property that has been described when, instead of the axes being rectangular, the primitive equation is referred to a pair of conjugate diameters as axes, provided, however, that the tubular ordinates be of elliptical transverse section such that their section by the plane of the two other coordinates is circular.

The extension of the method so as to represent an equation between three variables, referred to rectangular axes, presents no difficulties.

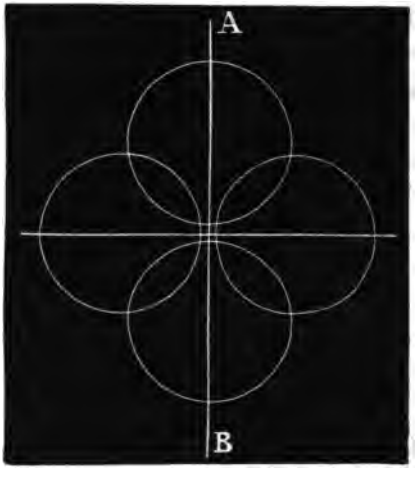
It will be observed that the envelope of the circular ordinates is the same central conic that we should obtain by taking, in the proper plane, ordinates affected with the sign $\sqrt{-1}$ as normals instead of ordinates, and measured from the extremities of the respective abscissæ which yield them.

One interesting consequence of taking account of both the representative curves of an equation is that it affords a geometrical illustration of the four-point intersection of pairs of conics in cases which are unintelligible without it; in the case of similar and similarly situated concentric ellipses, for example, the absence of any points of intersection on the ellipses is compensated for by the touching of the pair of supplementary hyperbolas on the coincident asymptotes*.

* The writer is under the impression that he will find that, in the general case of elimination between the equations of a pair of conics, if one or two

Years ago the writer became acquainted with the method in analytical geometry which regards a plane and a straight line, not as the pure abstractions of the mathematician, but as small portions of the surface of a sphere and of the periphery of a circle respectively in the limits in which the radii of the sphere and circle become infinite; and, whilst noting that these definitions were open to serious objections (chiefly as giving sides to a plane and a straight line of an unsymmetrical character), he was much struck by the extent

Fig. 2.



to which they enlarged the field in which geometrical interpretation might be consistently applied to algebraical

pairs of impossible values are given for the coordinates of the points of intersection, then as many pairs of real values will be given by elimination between the equations of the supplementary pair of conics which lie in the plane that contains the two centres; but he has yet to apply the analytical test of the correctness of this impression. The nature of this test will be to find whether for each case in which the primitive pair of equations give the squares of the ordinates negative, the supplementary pair give the squares of the ordinates positive, and *vice versâ*.

forms of expression. He became strongly impressed with the notion that fruitful speculation would lie in the direction indicated, of regarding a plane and a straight line as limits of some kind, and that kind such as would, if possible, cause them, at infinity of distance from the parts considered, to deviate symmetrically from the positions due to the abstract conceptions of a plane and a straight line. The rotation of the system of circles shown in figure 2 about the straight line AB as an axis will yield examples of a straight line and a plane of the symmetrical kind required, provided that the opposite pairs of equal circles are at the infinitesimal distance R from the central point of the figure, and that their radii are infinities of a second order in comparison with R; for, with these conditions, finite quantities, which are infinite in comparison with R, will be infinitesimal in comparison with the radii of the circles. Let a sphere of finite radius be described with its centre at the central point of the figure, then the part of the tube of which AB is the axis that is cut off by this sphere has the characteristics of our tubular ordinates, and the part of the same sphere cut off by the space between the upper and lower circles forms circles of the nature of our circular ordinates. When the sphere becomes infinite and of radius greater than the diameter of the circles of the figure, the infinite ordinates (tubular and circular, positive and negative) will all go out into space altogether without the system, in obvious analogy with the sudden passage, in certain otherwise continuous variations, of $+\infty$ and $-\infty$ into each other.

XXVI. *On the Cubic Integral* $u=\int_{\beta}^{\alpha}\frac{dx}{\sqrt{(a-x)(b-x)(c-x)}}$.

By Robert Rawson, Esq., Assoc. I. N. A., Hon. Memb. Manchester Literary and Philosophical Society, Memb. of the London Math. Society.

Read January 22nd, 1878.

1. As far as I know, the cubic integral has received but little attention from mathematicians; the reason for this may be that it has been regarded by them as a particular case of the quartic integral.

This, no doubt, is, to a certain extent, true. Still, the quartic integral is readily reduced to the cubic (see Art. 13), and there are some advantages in considering the cubic integral first, in its natural order, especially so in the reduction of it to an elementary integral of a fractional modulus n and amplitude θ.

The quartic integral here alluded to is usually called elliptic integral. I cannot hope, however, to induce mathematicians to abandon this unnatural terminology, viz. *elliptic integrals*, for the more natural one of *quartic integrals*, as suggested by Professor Cayley (see Salmon's 'Higher Algebra,' page 83).

In this paper I have not entered into the great question of the comparison of cubic integrals; my effort here has been of a more limited kind, viz. to reduce the cubic integral to the elementary integral,

$$u=\int_{0}^{\theta}\frac{d\theta}{\sqrt{1+n\cos m\theta}},$$

where n is a proper fraction, and m any positive whole number. I believe this elementary integral is better adapted to the purpose of tabulation than is the elementary integral used by Legendre and others, viz.

$$\int_0^\theta \frac{d\theta}{\sqrt{1-k^2\sin^2\theta}},$$

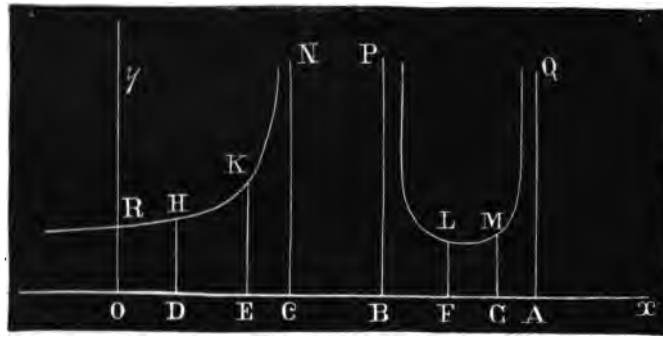
to which the former integral can be readily reduced. In tracing the curve whose polar equation is $r^2=2\times\sqrt{1+n\cos m\theta}$ by polar coordinates r, θ, the periodicity of cubic integrals is readily perceived.

And here I must express my surprise that this method of tracing the cubic integral has not been made use of in tracing elliptic functions. That this has not been done I infer from the circumstance of not finding it in Cayley's recent Treatise on Elliptic Functions.

2. Let a, b, c be positive, and taken in the order of magnitude, that is $a>b>c$.

If the roots are not positive, they can be readily made so by the linear transformation $x=A+z^2$.

Fig. 1.



With a view, therefore, of fixing palpably the values of the limits α, β, with respect to the roots a, b, c, it will be

necessary to trace by the usual rectangular-coordinate method, the curve whose equation is

$$y=\frac{1}{\sqrt{(a-x)(b-x)(c-x)}}. \quad . \quad . \quad . \quad (1)$$

Let Ox, Oy (fig. 1) be rectangular coordinates, origin at O.

When $x=0$, then $OR\sqrt{abc}=1$. The negative branch of the curve extends to infinity with a continually diminishing value of the positive ordinate. The positive branch extends only as far as A, where $OA=a$. All values of the ordinate y beyond this point are impossible. The ordinate y is impossible also between B and C, where $OB=b$ and $OC=c$, since the denominator of (1) is then negative.

When $x=a=OA$,	then $y=\infty$.
$x=b=OB$,	„ $y=\infty$.
$x=c=OC$,	„ $y=\infty$.
$x<a$ and $>b$,	„ y is real and positive.
$x<c$ and $>c$,	„ y is impossible.
$x<c$,	„ y is real and positive.

In this geometrical representation of the cubic integral there are two cases to consider, viz. :—

1. When the limits α and β are both less than c.
2. When the limits α and β lie between A and B.

In case 1 the cubic integral is correctly represented by the area HDEK, where $OE=\alpha$ and $OD=\beta$.

In case 2 the cubic integral is geometrically expressed by the area LFGM, where $OG=\alpha$ and $OF=\beta$.

3. Transformation of the cubic integral u into another cubic integral by the relation

$$x=\frac{2c-a+az}{1+z}=a-\frac{2(a-c)}{1+z}. \quad . \quad . \quad . \quad (2)$$

From (2), $dx=\frac{2(a-c)}{(1+z)^2}dz$, $\quad a-x=\frac{2(a-c)}{1+z}$,

$$b-x=\frac{a+b-2c}{1+z}\left\{1-\frac{(a-b)z}{a+b-2c}\right\}, \qquad c-x=\frac{a-c}{1+z}(1-z).$$

The limits become as follows :—

$$\text{When } x=\alpha, \text{ then } z=\frac{a+\alpha-2c}{a-\alpha};$$

$$x=\beta, \quad \text{„} \quad z=\frac{a+\beta-2c}{a-\beta}.$$

Substitute these values in the cubic integral u, and it becomes

$$u=\sqrt{\frac{2}{a+b-2c}}\int_{\frac{a+\beta-2c}{a-\beta}}^{\frac{a+\alpha-2c}{a-\alpha}}\frac{dz}{\sqrt{(1-z^2)\left(1-\frac{(a-b)z}{a+b-2c}\right)}}. \quad (3)$$

The value of $\sqrt{a+b-2c}$ is real, because $a+b-2c$ is positive.

The fraction $a-b$ upon $a+b-2c$ lies between 1 and zero; it is zero when $a=b$, and 1 when $b=c$. At each of these limits the cubic integral is soluble.

4. Transformation of the cubic integral (3) by the relation

$$-z=\cos m\theta, \quad \ldots\ldots \quad (4)$$

where m is any whole positive number.

From (4), $dz=m\sin m\theta d\theta=m\sqrt{1-z^2}d\theta$.

Substitute these values in (3), and it becomes

$$u=m\sqrt{\frac{2}{a+b-2c}}\int_{\frac{1}{m}\cos^{-1}\left(\frac{2c-\beta-\alpha}{\alpha-\beta}\right)}^{\frac{1}{m}\cos^{-1}\left(\frac{2c-\alpha-\alpha}{\alpha-\alpha}\right)}\frac{d\theta}{\sqrt{1+\frac{a-b}{a+b-2c}\cos m\theta}}. \quad (5)$$

Let u_1 represent the integral (5) between the limits $\alpha = c$ and $\beta = -\infty$, then

$$\left.\begin{aligned} u_1 &= \int_{-\infty}^{c} \frac{dx}{\sqrt{(a-x)(b-x)(c-x)}} \\ &= m\sqrt{\frac{2}{a+b-2c}} \int_0^{\frac{\pi}{m}} \frac{d\theta}{\sqrt{1+\frac{a-b}{a+b-2a}\cos m\theta}} \end{aligned}\right\} . \quad (6)$$

5. The integral in equation (5) applies only to case 1 in art. 2; the second case requires a slightly different transformation, as follows :—

Transform the cubic integral u into another cubic integral by the relation

$$x = \frac{a+b}{2} + \frac{(a-b)z}{2} . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

From (7), $dx = \frac{a-b}{2} dz$, $\qquad a-x = \frac{a-b}{2}(1-z)$,

$$-(b-x) = \frac{a-b}{2}(1+z),$$

$$-(c-x) = \frac{a+b-2c}{2}\left(1+\frac{(a-b)z}{a+b-2c}\right).$$

The limits become :—

when $x=\alpha$, $z = \frac{2\alpha-a-b}{a-b}$;

when $x=\beta$, $z = \frac{2\beta-a-b}{a-b}$.

Substitute these values in the cubic integral u, then

$$u = \sqrt{\frac{2}{a+b-2c}} \int_{\frac{2\beta-a-b}{a-b}}^{\frac{2\alpha-a-b}{a-b}} \frac{dz}{\sqrt{(1-z^2)\left(1+\frac{(a-b)z}{a+b-2c}\right)}} . \quad . \quad (8)$$

6. Transformation of the cubic integral (8) by the relation

$$z=\cos m\theta, \quad \ldots\ldots\ldots \quad (9)$$

where m is any positive whole number.

From (8), $dz=-m\sin\theta d\theta=-m\sqrt{1-z^2}d\theta$; then (8) becomes

$$u=m\sqrt{\frac{2}{a+b-2c}}\int_{\frac{1}{m}\cos^{-1}\left(\frac{2\beta-a-b}{a-b}\right)}^{\frac{1}{m}\cos^{-1}\left(\frac{2\alpha-a-b}{a-b}\right)}\frac{d\theta}{\sqrt{1+\frac{a-b}{a+b-2c}\cos m\theta}}. \quad (10)$$

Let u_2 represent the integral (10) between the limits $\alpha=a$ and $\beta=b$, then

$$\left.\begin{aligned} u_2&=\int_b^a\frac{dx}{\sqrt{(a-x)(b-x)(c-x)}}\\ &=m\sqrt{\frac{2}{a+b-2c}}\int_0^{\frac{\pi}{m}}\frac{d\theta}{\sqrt{1+\frac{a-b}{a+b-2c}\cos m\theta}}\end{aligned}\right\}. \quad (11)$$

By comparing (6) and (11) we obtain :—

$$\int_b^a\frac{dx}{\sqrt{(a-x)(b-x)(c-x)}}=\int_{-\infty}^{c}\frac{dx}{\sqrt{(a-x)(b-x)(c-x)}}. \quad (12)$$

This result is neat, and gives an answer to question 5508 of the 'Educational Times' for December 1877.

7. Since $2\alpha<2a$, then $2\alpha-a-b<a-b$; therefore $2\alpha-a-b$ upon $a-b$ and $2\beta-a-b$ upon $a-b$ are always proper fractions, whatever may be the values of the real positive roots a, b, c.

The limits of both (5) and (10), integrals which represent the cases in 1 and 2 in art. 2 respectively, are angles θ_1 and θ_2, which lie between zero and π.

Hence, in both cases refered to in art. 2, the following ntegral will apply :—

$$u=m\sqrt{\frac{2}{a+b-2c}}\int_{\frac{\theta_2}{m}}^{\frac{\theta_1}{m}}\frac{d\theta}{\sqrt{1+n\cos m\theta}}, \quad . \quad (13)$$

where $n=\frac{a-b}{a+b-2c}$, $\cos\theta_1=\frac{2c-a-\alpha}{a-\alpha}$ or $\frac{2\beta-a-b}{a-b}$,

and $\cos\theta_2=\frac{2c-a-\beta}{a-\beta}$ or $\frac{2\alpha-a-b}{a-b}$.

8. It appears, therefore, that if the elementary integral

$$\int_0^{\theta}\frac{d\theta}{\sqrt{1+n\cos m\theta}} \quad . \quad . \quad . \quad . \quad . \quad (14)$$

is calculated, for any given value of the modulus n, from $\theta=$zero to $\theta=\frac{\pi}{m}$, the result will be a complete solution (arithmetical) of the cubic integral in the case when all the roots are real. The following investigation will show that the same elementary integral will apply equally well when two of the roots are unreal.

9. In the case of the cubic integral having only one real root it assumes the form

$$v=\int_{\beta}^{\alpha}\frac{dx}{\sqrt{(a-x)\{(x-b)^2+c^2\}}}. \quad . \quad . \quad (15)$$

where a, b, c are independent constants. The limits α and β cannot be greater than a.

Transform the cubic integral (15) by the relation

$$x=\frac{A-B\cos\frac{m\theta}{2}}{1-\cos\frac{m\theta}{2}}, \quad . \quad . \quad . \quad . \quad (16)$$

where $A=a-\sqrt{(a-b)^2+c^2}$, $B=a+\sqrt{(a-b)^2+c^2}$.

From (16), $$dx = m\sqrt{(a-b)^2+c^2}\,\frac{\sqrt{1-\cos^2\frac{m\theta}{2}}}{\left(1-\cos\frac{m\theta}{2}\right)}\,d\theta;$$

$$a-x = \sqrt{(a-b)^2+c^2}\,\frac{1+\cos\frac{m\theta}{2}}{1-\cos\frac{m\theta}{2}};$$

$$(x-b)^2+c^2 = \frac{\sqrt{(a-b)^2+c^2}\{3\sqrt{(a-b)^2+c^2}+b-a\}}{\left(1-\cos\frac{m\theta}{2}\right)^2}$$

$$\left\{1+\frac{\left(\sqrt{(a-b)^2+c^2}+a-b\right)\cos m\theta}{3\sqrt{(a-b)^2+c^2}+b-a}\right\}.$$

Substitute these values in (15), observing the change in the limits, then

$$v = \frac{m}{\sqrt{3\sqrt{(a-b)^2+c^2}+b-a}} \times \left\{ \int_{\frac{2}{m}\cos^{-1}\left(\frac{a-\alpha-\sqrt{(a-b)^2+c^2}}{a-\alpha+\sqrt{(a-b)^2+c^2}}\right)}^{\frac{2}{m}\cos^{-1}\left(\frac{a-\beta-\sqrt{(a-b)^2+c^2}}{a-\beta+\sqrt{(a-b)^2+c^2}}\right)} \frac{d\theta}{\sqrt{1+\frac{\sqrt{(a-b)^2+c^2}+a-b}{3\sqrt{(a-b)^2+c^2}+b-a}\cos m\theta}} \right\}. \quad (17)$$

It is readily seen that $3\sqrt{(a-b)^2+c^2}+b-a$ is a positive quantity, whatever may be the values of a, b, c. The coefficient of $\cos m\theta$ is a proper fraction; therefore equation (17) may be written as follows:—

$$v = \frac{m}{\sqrt{3\sqrt{(a-b)^2+c^2}+b-a}} \int_{\frac{\theta_2}{m}}^{\frac{\theta_1}{m}} \frac{d\theta}{\sqrt{1+n\cos m\theta}}, \quad (18)$$

where $$n=\frac{\sqrt{(a-b)^2+c^2}+a-b}{3\sqrt{(a-b)^2+c^2}+b-a},$$

$$\cos\frac{\theta_1}{2}=\frac{a-\alpha-\sqrt{(a-b)^2+c^2}}{a-\alpha+\sqrt{(a-b)^2+c^2}},$$

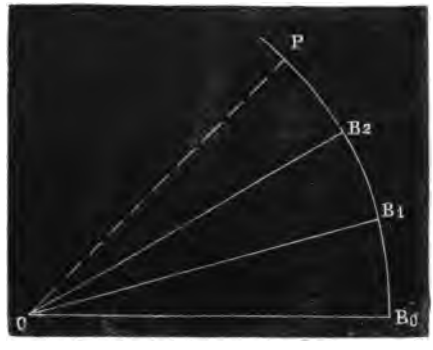
and $$\cos\frac{\theta}{2}=\frac{a-\beta-\sqrt{(a-b)^2+c^2}}{a-\beta+\sqrt{(a-b)^2+c^2}}.$$

10. The comparison of the cubic elementary integral, viz.

$$\int_0^\theta \frac{d\theta}{\sqrt{1+n\cos m\theta}}, \quad \ldots\ldots \quad (19)$$

with the elliptic functions of the first order will be considered hereafter. It will be necessary now to show the march of the integral (19) with respect to polar coordinates in the usual way.

Fig. 2.



Let B_0P (fig. 2) be the polar curve traced by the equation

$$r=\frac{\sqrt{2}}{\{1+n\cos m\theta\}^{\frac{1}{4}}},$$

where $r=OP$, and $\theta=B_0OP$.

To trace the curve B_0P :—

When θ=zero, $OB_0=\frac{\sqrt{2}}{(1+n)^{\frac{1}{4}}}$;

when $\theta=\frac{\pi}{m}=B_0OB_1$, $OB_1=\frac{\sqrt{2}}{(1-n)^{\frac{1}{4}}}$;

when $\theta=\frac{2\pi}{m}=B_0OB_2$, $OB_2=\frac{\sqrt{2}}{(1+n)^{\frac{1}{4}}}$;

when $\theta=\frac{3\pi}{m}=B_0OB_3$, $OB_3=\frac{\sqrt{2}}{(1-n)^{\frac{1}{4}}}$;

&c. &c.

It is readily seen that the polar radii from OB_2 to OB_4 are exactly the same as the polar radii included in the angle B_0OB_2, because $\cos m\left(\frac{2\pi}{m}+\varphi\right)=\cos m\varphi$.

The polar radii on each side of OB_1 are equal at the same angular amplitude, because $\cos m\left(\varphi+\frac{\pi}{m}\right)=\cos m\left(\frac{\pi}{m}-\varphi\right)$; therefore the polar area B_0OB_1 is equal to the polar area B_1OB_2.

From the above considerations the principle of the periodicity of cubic integrals is readily perceived, and it follows that

$$p\int_0^{\frac{\pi}{m}}\frac{d\theta}{\sqrt{1+n\cos m\theta}}=\int_0^{\frac{p\pi}{m}}\frac{d\theta}{\sqrt{1+n\cos m\theta}},$$

where p is any whole number. If the functional notation be used, then

$$pf\left(n,\frac{\pi}{m}\right)=f\left(n,\frac{p\pi}{m}\right). \quad . \quad . \quad . \quad . \quad (20)$$

From the figure it will be seen that

$$f\left\{n,\left(\frac{2p\pi}{m}+\varphi\right)\right\}-f(n,\varphi)=2f\left(n,\frac{\pi}{m}\right),\quad\ldots\quad(21)$$

$$f\left\{n,\left(\frac{2p\pi}{m}-\varphi\right)\right\}+f(n,\varphi)=2f\left(n,\frac{\pi}{m}\right);\quad\ldots\quad(22)$$

$$\therefore f\left\{n,\left(\frac{2p\pi}{m}-\varphi\right)\right\}+2f(n,\varphi)=f\left\{n,\left(\frac{2p\pi}{m}+\varphi\right)\right\},\quad(23)$$

where φ is less than $\frac{\pi}{m}$.

11. Approximate formula for calculating the cubic integral:—

Put $\theta_1=\frac{\pi}{m}$, r_0, r_1, r_2, &c., the polar radii corresponding to the angles zero, $\frac{\pi}{pm}$, $\frac{2\pi}{pm}$, &c.

It can be readily shown that

$$\int_0^{\frac{\pi}{m}}\frac{d\theta}{\sqrt{1+n\cos m\theta}}$$

$$=\frac{\pi}{6mp}\left\{r_0^2+r_p^2+4(r_1^2+r_3^2+\ldots r_{p-1}^2)+2(r_2^2+\ldots r_{p-2}^2)\right\},\quad(24)$$

where p is any whole number.

This formula is obtained by supposing the curve B_0P to coincide with the curve whose equation is $r^2=a_0+a_1\theta+a_2\theta^2$. The values of r_0, r_1, &c. are as follows:—

$$r_0^2=\frac{2}{\sqrt{1+n}};\qquad r_1^2=\frac{2}{\sqrt{1+n\cos\frac{\pi}{p}}};$$

$$r_2^2=\frac{2}{\sqrt{1+n\cos\frac{2\pi}{p}}},\qquad r_3^2=\frac{2}{\sqrt{1+n\cos\frac{3\pi}{p}}};$$

. . . . . .

$$r_{p-1}^2 = \frac{2}{\sqrt{1+n\cos\frac{\pi}{p}}}; \qquad r^2 = \frac{2}{\sqrt{1-n}}.$$

The larger p is, the more accurate is formula (24).

12. The reduction of the quartic integral, usually called elliptical integral, to the cubic integral is remarkably easy.

To effect this, let the quartic integral be as follows :—

$$u = \int_{\beta}^{\alpha} \frac{dy}{\sqrt{(e-y)(f-y)(g-y)(h-y)}}, \quad . \quad . \quad (25)$$

where e, f, g, h are in the order of magnitude.

Transform the integral (25) by the relation

$$y = \frac{p+ex}{q+x} = e + \frac{p-eq}{q+x}. \quad . \quad . \quad . \quad . \quad (26)$$

From (26),

$$dy = \frac{eq-p}{(q+x)^2}dx, \qquad e-y = \frac{eq-p}{q+x},$$

$$f-y = \frac{e-f}{q+x}\left(\frac{fq-p}{e-f} - x\right), \qquad g-y = \frac{e-g}{q+x}\left(\frac{gq-p}{e-g} - x\right),$$

$$h-y = \frac{e-h}{q+x}\left(\frac{hq-p}{e-h} - x\right).$$

Substitute these values in (25), observing the limits, then

$$u = \sqrt{\frac{eq-p}{(e-f)(e-g)(e-h)}} \times \int_{\frac{\beta q-p}{e-\beta}}^{\frac{\alpha q-p}{e-\alpha}} \frac{dx}{\sqrt{\left(\frac{fq-p}{e-f} - x\right)\left(\frac{gq-p}{e-g} - x\right)\left(\frac{hq-p}{e-h} - x\right)}}. \quad . \quad (27)$$

This integral only requires that $eq > p$ in order to obtain its arithmetical value.

If $p=0$, then (27) becomes

$$u=\sqrt{\frac{eq}{(e-f)(e-g)(e-h)}}\times \int_{\frac{\beta q}{e-\beta}}^{\frac{\alpha q}{e-\alpha}} \frac{dx}{\sqrt{\left(\frac{fq}{e-f}-x\right)\left(\frac{gq}{e-g}-x\right)\left(\frac{hq}{e-h}-x\right)}}, \quad . \quad (28)$$

which is a cubic integral whose roots can be compared with (1). If the roots g and h of equation (25) are impossible, then equation (28) becomes a particular case of the equation considered in art. 9.

If all the roots in equation (25) are impossible, then it can be readily reduced to the form

$$u=\int_{\beta}^{\alpha} \frac{dy}{\sqrt{(1+y^2)(1+a^2y^2)}} \quad . \quad . \quad . \quad . \quad (29)$$

Transform (29) by the relation

$$y^2=\frac{1-\cos m\theta}{1+\cos m\theta}; \quad . \quad . \quad . \quad . \quad . \quad (30)$$

$$\therefore u=\frac{m}{\sqrt{2(+a^2)}} \int_{\frac{1}{m}\cos^{-1}\left(\frac{1-\beta^2}{1+\beta^2}\right)}^{\frac{1}{m}\cos^{-1}\left(\frac{1-\alpha^2}{1+\alpha^2}\right)} \frac{d\theta}{\sqrt{1+\frac{1-a^2}{1+a^2}\cos m\vartheta}} \quad . \quad . \quad . \quad . \quad (31)$$

The equation (31) is therefore of the form given in art. 7.

XXVII.—*Notice of a Fossil Plant found at Laxey, in the Isle of Man.* By E. W. Binney, F.R.S., F.G.S., President.

Read January 22nd, 1878.

Many years ago the late Sir William Logan drew attention to the occurrence of fossil plants in the Devonian strata of Canada; and Professor J. W. Dawson, F.R.S., in the 'Quarterly Journal of the Geological Society,' in vols. xv. and xviii., described and figured some of these specimens. Amongst them was a plant which he designated *Psilophytum*. Dr. S. S. Scoville has since discovered the remains of plants in the Lower Silurians at Longstreet Creek, near Lebanon, Ohio, which Professor Newbury regarded as the casts of some large fucoids or marine plants. Count Saporta has found the branch of a fern in the Silurian schists or slates of Angers, France. Professor Leo Lesquereux, to whom we owe so much for his labours in investigating the fossil plants of the United States, in a paper read before the American Philosophical Society, October 10th, 1877, has described and figured a plant from the Lower Helderberg Sandstone, Michigan, under the name of *Psilophytum cornutum*.

In a paper read by myself before this Society on the 26th December, 1876, I stated that, after some years' search, I had not been able to find the *Palæochorda major* mentioned by Professors Harkness and Nicholson as occurring in the Manx schists, in such a state of preservation as to be certain of its true nature, but I had a fucoid in my posession found by Mr. Grindlay in the drift near Laxey.

As Mr. Lesquereux's so much resembles the one found at Laxey, I shall give his description at length :—

"*Psilophytum cornutum*, sp. nov. Plate 1. fig. 1.

"Stem thick, dichotomous; divisions variable in distance, the terminal ones short, pointed, nearly equal in size and length, surface slightly rugose and irregularly striate.

"The branches in the lower part are thick comparatively to their length, three or four millimetres, irregularly striate when decorticated, or merely punctuate upon the thin bark, with small projecting dots resembling the basilar remains of scales or small decayed leaves; lateral branches short, narrowed to a sharp point; the upper or terminal ones about equal in length, appearing like a pair of pointed horns."

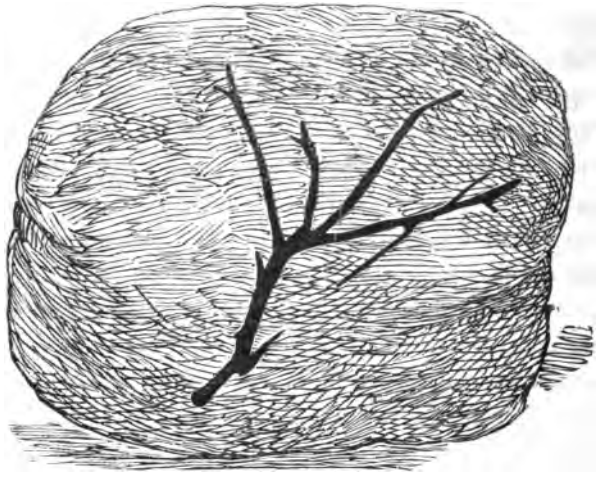
The species is only comparable to some of the fragments not specified but figured by Prof. J. W. Dawson (Geol. Survey of Canada, "Fossil Plants of the Devonian and Upper Silurian Formations," figs. 243, 244). The author remarks "that these fragments are probably originating in the Upper Silurian of Gaspé; that, as they are found in the lower part of the limestone which underlies the Devonian Gaspé Sandstone and become more abundant in the upper beds, this suffices to indicate the existence of the neighbouring land, probably composed of Silurian rocks and supporting vegetation.

"From the preservation of its branches, even to the smallest subdivisions, the specimens here represent part of a plant imbedded in the place of its growth. The matrix is a piece of very hard calcareous shale seven or eight millimetres thick, bearing on one side irregular undulations like ripple-marks, without any trace of organic remains, and on the other the fragments of plants as figured here (p. 216). The branch in (*a*) represents a different

species, and, indeed, a marine or, rather, a brackish plant, closely related to the species of the present genus *Chorda*, Stack. This fragment seems to have been mixed in the tide-pools with freshwater or land plants growing there; for another thick specimen of the same locality and compound bears a profusion of marine mollusks, and has only branches of this as yet undescribed marine species, *Calamophycus septus*.

"*Habitat*. Lower Helderberg Sandstone, Michigan; discovered and communicated by Dr. Carl Rominger (State Geologist)."

The woodcut below represents the specimen a little over the natural size.



On comparing my Manx specimen, which was found on the surface in a field at Laxey, with that figured and described by Prof. Lesquereux; it agrees with the latter in every respect, except that striæ and scales are not observable on the stem. The stem is thick, dichotomous; divisions variable in distance, the terminal ones short,

pointed, nearly equal in size and length, surface nearly smooth. The branches in the lower part are thick comparatively to their length. The surface of the stem appears to be smooth, and affords no evidence of striæ or scales.

The stone in which the plant is imbedded is a fine-grained grit of a grey colour; and the specimen itself is of a yellow tint, as if coloured by oxide of iron; it runs nearly at right angles to the bedding of the stone, and appears as if standing in the same position in which it had grown. The stone is a rolled one, but it is evidently from the Manx schists found in the vicinity. These, according to Professors Harkness and Nicholson, are of the age of the Skiddaw slates; but the rock in which the fossil occurs may be of older date, as some of the lower portions of the series have not yet been clearly determined. Here, therefore, we have evidence of a plant in the lowest part of the Silurian formation, or even lower. By diligent search the rock in which the specimen occurs may probably be found *in situ* in the upper part of the Laxey valley. The great resemblance (if not identity) of the Manx to the American specimen is very remarkable, and shows the similarity of conditions then prevailing in distant parts of the globe. The specimen might have been called *Psilophytum cornutum*, if any marking on the surface of the stem had been observed; but as these appear to be absent, it is proposed to call it *Psilophytum Monense*. As to the nature of the water in which it grew, there is no evidence from organic remains; but in its characters it more resembles a Fucoid than a land plant.

XXVIII. *On Indigo-Blue from* Polygonum tinctorium *and other Plants.* By EDWARD SCHUNCK, Ph.D., F.R.S.

Read April 16th, 1878.

SOME papers read before the Society many years ago, and subsequently published in its 'Memoirs'*, contain an account of my experiments with the leaves of the *Isatis tinctoria*, or common woad, the well-known plant employed in Europe for dyeing blue before the introduction of indigo from the East. I showed that the leaves of this plant do not, as some have supposed, contain either indigo-blue or its hydride ready formed, but yield by careful treatment a peculiar glucoside—indican—which, when acted on by acids and other reagents, splits up into indigo-blue and indigo-glucine, the latter being a body resembling glucose. My experiments also show that this substance, indican, is a highly unstable body, undergoing when its watery solution is heated for some time, or, more rapidly, by the action of caustic alkalies, an entire change, on the completion of which it no longer yields indigo-blue by decomposition with acids, but in place of the latter gives indigo-red, indifulvine, leucine, and other products. Though I succeeded in ascertaining the composition of indican and the relation in which it stands to indigo-blue, the difficulty of obtaining large quantities of it in consequence of its excessive liability to change, prevented my proceeding further with the investigation. It seemed to me, however, that it might be of some interest to ascertain whether other indigo-

* 'Memoirs,' ser. 2, vol. xii. p. 177, and vol. xiv. p. 181.

yielding plants contain ready-formed indigo-blue (as has been maintained with so much persistence), or whether the colouring-matter exists in the vegetable cells in the form of indican or some other glucoside; and I have accordingly examined such of the plants known to give indigo as I have been able to procure.

Before stating the results to which I wish to direct attention on this occasion, I may mention an observation belonging, strictly speaking, to the part of the subject previously treated of, which, however, I will now describe in a few words, as I may not have another opportunity of doing so. In my last memoir I stated that, among the products of decomposition of indican from *Isatis tinctoria*, leucine is usually found, sometimes, indeed, in considerable quantities. In some more recent experiments made with woad leaves I obtained, besides leucine, a substance having all the properties of tyrosine. This substance was only slightly soluble in cold water, but soluble in boiling water, from which it separated on cooling in long needles, forming, when dry, a snow-white felted mass. Its watery solution gave the well-known reaction with mercuric nitrate. Its solution in concentrated sulphuric acid, after neutralization with barium carbonate, gave a purple colour with ferric chloride. According to Prof. Gamgee, who had the kindness to examine the substance for me, it showed under the microscope the forms characteristic of ordinary tyrosine. There could be no doubt, therefore, of its identity with the latter. As the tyrosine in this case was not obtained from pure indican, but from the crude alcoholic extract of the leaves, it is impossible to say whether it preexisted in the plant or whether it was a product of decomposition of the indican of the extract. The latter supposition is the most probable one; for tyrosine, being almost insoluble in alcohol, could hardly be contained

in any appreciable quantity in the alcoholic extract of the leaves.

Some connexion between tyrosine and indigo-blue has frequently been suspected by chemists. Indeed a glance at the formulæ of the two bodies will show that some connexion is possible, since, by replacing H by CH_3 in indigo-blue and adding $2H_2O$, we arrive at the formula of tyrosine, thus :—

$$\underset{\text{Indigo-blue.}}{C_8H_5NO} + CH_2 + 2H_2O = \underset{\text{Tyrosine.}}{C_9H_{11}NO_3}.$$

In order to explain the formation of tyrosine from indican, we may suppose the latter to split up into tyrosine, indiglucine, acetic acid, and carbonic dioxide, thus :—

$$\underset{\text{Indican.}}{C_{26}H_{33}NO_{18}} + 3H_2O = \underset{\text{Tyrosine.}}{C_9H_{11}NO_3} + \underset{\text{Indiglucine.}}{2(C_6H_{10}O_6)}$$
$$+ 2(C_2H_4O_2) + CO_2.$$

Supposing the tyrosine in this case to have been really formed from indican, the question suggests itself whether the leucine and tyrosine so frequently found in the animal organism as products of disease, may not be derived from some substance similar to indican rather than directly and immediately from albuminoids.

Polygonum tinctorium.

This plant has long been known and employed as a source of indigo by the Chinese. According to Stanislas Julien*, who has given translations from Chinese works of various processes for extracting indigo from the leaves, the plant is called in China Lân, the most productive variety being termed Tcha-Lân, *i. e.* the Lân resembling the tea shrub. It was introduced into Europe in the eighteenth

* Comptes Rendus, t. vii. p. 703

century, and at one time, particularly about the years 1838 to 1840, formed the subject of numerous investigations by eminent French botanists and chemists, such as Turpin, Joly, Baudrimont, Pelletier, Robiquet, and others, some hopes being entertained that the plant might be cultivated profitably in France. The numerous trials made with this view having led to no result, the matter fell again into oblivion, and this interesting plant remained what it was before, a mere curiosity.

I obtained the seeds of the plant from Messrs. Vilmorin, Andrieux & Co., the eminent horticulturists of Paris, and therefore felt sure of their genuineness. They were sown in a hotbed, and germinated rapidly. As soon as the young plants were a few inches high they were transplanted into the open ground, where they grew vigorously, producing an abundance of leaves and attaining during the summer season a height of nearly three feet. Towards the end of summer spikes of pretty pink flowers, resembling those of other species of *Polygonum*, made their appearance. The seed, however, did not ripen in the open air, the plant being cut down by the early frosts before this could take place; but, by growing a few plants under glass, I obtained a quantity of well-matured seeds, which yielded another crop in the following season.

For a botanical description of *Polygonum tinctorium* I must refer to Turpin* and Joly†, the latter of whom has given a full account of its structure and affinities. Of the various organs, the leaves, being the seat of the blue colouring-matter, are alone of any interest to the chemist. These leaves, which are large, oval in shape, and glossy, show no indication of the presence within their tissue of any pigment beside the chlorophyll to which they owe

* Comtes Rendus, t. vii. p. 806.

† 'Sur le *Polygonum tinctorium*:' Montpellier, 1839.

their lively green colour, except, occasionally, in certain places where they have suffered injury from the bites of insects or from other causes, and where blue spots make their appearance—a phenomenon which I shall endeavour to explain presently. It is nevertheless easy to show that they contain a considerable quantity of what the French call a "matière colorable," *i. e.* a substance which, though colourless in itself, yields colouring-matter by appropriate treatment. A few leaves having been cut into pieces and rubbed up with a little water in a mortar to a thin paste, the mass is poured on a bit of calico, and yields, by squeezing and kneading, a green muddy liquid which, on the addition of a little sugar-of-lead solution, gives a green flocculent precipitate containing the chlorophyll, albumen, and other matters previously held in suspension. The liquid filtered from this precipitate is clear and yellow, and, on being mixed with sulphuric or hydrochloric acid and left to stand for several hours, yields a deposit consisting of tolerably pure indigo-blue. The amount of colouring-matter obtained in this way from *Polygonum tinctorium* is far greater than that which the same quantity of woad-leaves grown in the same locality would produce, proving that the yield is influenced not only by soil and climate, but also by the peculiar nature of the plant.

The isolation of the "matière colorable" of *Polygonum tinctorium* is still not a very easy task. The same precautions must be observed as in the case of *Isatis tinctoria*, particularly as regards the evaporation of the solvents employed, which must always be effected without applying artificial heat. Unless some means are at disposal for evaporating rapidly at the ordinary temperature, by means of a current of air or otherwise, success is very uncertain. The method formerly employed in preparing indican from *Isatis tinctoria* was first tried. The leaves of the plant were

dried in a stove moderately heated, and, while still warm, ground to powder. The powder, which had the colour of fresh hay, was passed through a hair sieve to separate the leaf-stalks and other fibrous portions, and then extracted in a percolator with spirit of wine. The green alcoholic extract was evaporated in a shallow tin dish, the evaporation being assisted by passing a current of air over the surface of the liquid in the apparatus formerly described. Chlorophyll and fatty matter were deposited during evaporation, leaving a brown watery liquid, which was poured off, agitated with freshly precipitated copper oxide, and filtered. The copper in the filtrate was precipitated with sulphuretted hydrogen, and the filtered liquid was evaporated as before. The residue was treated with absolute ether, which dissolved a portion, and left, on evaporation, a yellow sirup. This sirup is the indigo-producing body as pure as it is possible to obtain it. I prefer to this the following process, as being more expeditious and surer. The alcoholic extract of the dried leaves having been evaporated, the watery liquid which is left is mixed with acetate-of-lead solution, which gives a dirty-yellow precipitate, consisting of chlorophyll and other impurities in combination with lead. To the clear yellow filtrate basic lead acetate is added; this gives a primrose-yellow precipitate, which is filtered off and, after being washed with water and then with alcohol, is suspended in absolute alcohol, through which a current of carbonic anhydride is passed. After the gas has passed through for some time the liquid acquires a yellow colour, and, after being filtered from the insoluble portion, consisting principally of lead carbonate, is evaporated at the ordinary temperature. Water added to the residue leaves a portion undissolved, which is filtered off. Sulphuretted hydrogen is passed through the filtrate to precipitate the lead contained in it; and having been again

filtered, it is evaporated, when it leaves a sirupy residue which may be treated with ether as before.

The indigo-producing body thus obtained is, if it be permitted to draw a certain conclusion from mere qualitative reactions, identical with the indican of *Isatis tinctoria*. Its appearance is that of a yellow transparent sirup, showing no tendency to assume a crystalline form. It is soluble in water, alcohol, and ether. The watery solution has a more or less acid reaction. It becomes of a deep yellow colour on the addition of caustic alkali, and gives with basic lead acetate a light-yellow precipitate. When the watery solution is mixed with a little sulphuric or hydrochloric acid and left to stand for some time, the surface of the liquid becomes covered with a film of indigo-blue, a deposit of the same substance being usually formed at the bottom. The filtered liquid shows, when tested with a salt of copper and an excess of caustic alkali, the well-known reaction of glucose. If, however, the watery solution is left to stand at the ordinary temperature for a considerable time, or if it is simply boiled for some time, or if it is mixed with caustic alkali and left for a short time, it no longer yields indigo-blue on the addition of acid. This is probably due, as I have shown to be the case with indican, to a molecular change, resulting, when completed, in the formation of a body which, when decomposed with acids, yields indirubine and brown resinous substances in place of indigo-blue. When a large quantity of watery solution is mixed with acid and left to stand, a portion of the substances undergoes, it seems, the same change; for the deposit formed when operating with one litre or more of the solution contains not only indigo-blue, but also indirubine, indifulvine, and other products. The deposit, which in this case is almost black, after being filtered off, washed, and dried, is treated first with caustic alkali and then with cold

alcohol, in order to remove the indifulvine and other resinous substances. On treating the residue with boiling alcohol, the indirubine dissolves, and is obtained, after several crystallizations, in the beautiful dark-red needles characteristic of the substance. The portion insoluble in boiling alcohol is indigo-blue, requiring for its purification merely to be dissolved in some suitable menstruum, such as boiling aniline. I am inclined to think that the same molecular change takes place in the cells of the plant during the later stages of its development; for I obtained from some leaves gathered late in the season when the flowers had begun to appear, a quantity of indican having the usual appearance, but giving, by decomposition with acid, far less indigo-blue and more indirubine and other products than the indican from younger leaves. This result was confirmed by experiments, to be described presently, made with the leaves themselves.

The preceding experiments lead to the conclusion that the leaves of *Polygonum tinctorium* contain a substance not to be distinguished from the indican of *Isatis tinctoria*, which, by decomposition with acids, yields indigo-blue and glucose, accompanied by some by-products, and that there is no proof of the existence of ready-formed colouring-matter in the plant while the latter is living and in a healthy state. The preexistence of indigo-blue, or of its hydride, indigo-white, in these plants was taken for granted forty or fifty years ago, when the class of bodies which we now call glucosides and the peculiar kind of decomposition which they undergo were unknown. Even now a superficial examination of some phenomena would almost certainly lead to the conclusion that the indigo-blue is formed by the action of air, *i. e.* in consequence of the oxidation of some easily oxidizable substance in the plant. Bearing in mind, however, with what extreme facility indican is de-

composed, its watery solution on standing some time, even at the ordinary temperature, depositing indigo-blue, I think it will not be difficult to explain all the phenomena hitherto observed by myself and others.

On taking a plant of *Polygonum tinctorium* and making incisions with a penknife in the leaves between the main vessels, or crushing the soft parts of the leaves here and there with an agate pestle, then, after a short time, plunging the whole plant into boiling alcohol to remove the chlorophyll, it will be found that those parts of the leaf which have not been injured become white or retain only a faint yellow tinge, while those parts that have been cut, crushed, or otherwise injured, show a blue colour, the coloration extending for some distance inwards from the place where the lesion occurred, the most intense colour being at the edge of the cut or bruise. So, too, in the living plant, when some injury accidentally occurs to a leaf, the part injured will appear blue. Nothing can be more natural than to suppose that in these cases the blue colouring-matter is formed by the action of the air, *i. e.* by the oxidation of some substance which escapes from the cells in consequence of organic lesion, just as the surface of a freshly cut apple or pear becomes brown on exposure.

If a plant of *Polygonum tinctorium* be immersed in water, and the water be frozen by surrounding the vessel containing it with a freezing-mixture, it will be found, after complete thawing, that the leaves or parts of leaves which have been thoroughly frozen appear of a dark colour and are quite flaccid; and if the plant be then immersed in boiling alcohol so as to dissolve the chlorophyll and other matters, those very parts show afterwards an intense blue colour, while those portions which had remained unfrozen appear almost colourless. This experiment, which had already been made by Joly, was considered by him to prove

the preexistence of indigo-blue in the plant—though why, if this were the case, the colouring-matter should not make its appearance in the unfrozen portions of the leaf, I am at a loss to understand.

The fact that a fresh leaf of *Polygonum tinctorium*, if immersed in alcohol or ether, appears blue after the chlorophyll has been removed, has also been considered to prove the preexistence of indigo-blue in the cells. This phenomenon is always observed when the leaves are immersed in cold alcohol, and more distinctly when ordinaary spirits of wine are taken than with absolute alcohol. A very simple experiment suffices, however, to prove that in this, as in all the other cases, appearances are deceptive. If freshly gathered leaves of *Polygonum tinctorium* are plunged at once not into cold, but into boiling alcohol, the whole of the colour is soon removed, the leaves retaining only a faint yellow tinge. On now simply evaporating the green alcoholic liquid, not a trace of indigo-blue will be found in the residue. It is therefore absolutely certain that the leaves contain no ready-formed colouring-matter; for so stable a body as indigo-blue could not possibly be decomposed or be made to disappear by the action of boiling alcohol only. It must necessarily appear either in the alcoholic extract of the leaves or in the residual portion left by the alcohol.

A very simple explanation offers itself, I think, for all the phenomena hitherto observed. Indican, the mother substance of indigo-blue, is a body the molecules of which are in a state of unstable equilibrium. As long as it is contained within the cells of the plant the vitality of the cells keeps it in its original unchanged condition. As soon, however as this vitality is destroyed (whether by organic lesion, by extreme cold, or any other means), the indican begins to undergo decomposition, the molecules

rearrange themselves in the order to which their chemical affinities predispose them, and the compound splits up into indigo-blue and indiglucine, this taking place so rapidly that in certain cases it would appear as if the colouring-matter preexisted in the plant. If (to take the simplest case) the leaves as soon as gathered are immersed in cold spirits of wine or in cold ether, the vitality of the cells is thereby destroyed; and the indican contained in them is then in part decomposed, yielding indigo-blue, which remains undissolved, imparting a more or less distinct blue tint to the leaves. When boiling spirit of wine is taken, the indican is extracted before it can undergo decomposition, and dissolves in the spirit. It may be detected in the residue obtained on spontaneous evaporation of the alcoholic extract by its property of yielding indigo-blue on decomposition with acids, as above described. It is possible that the leaves contain some ferment which hastens the decomposition of the indican as soon as vitality has ceased; but I have no positive evidence to offer in favour of this view.

I may, in conclusion, describe another experiment, which, though it teaches nothing new, confirms what I have just stated, and is interesting in its way. Having cut some sprigs of *Polygonum tinctorium*, about six inches long, I immersed the cut ends in dilute hydrochloric acid (consisting of one part of acid of specific gravity 1·15 and ten parts of water), and left them to stand for several days exposed to the sun and air. The acid was gradually absorbed, ascending through the stems, first into the lower leaves, then into the higher ones. The gradual absorption of the acid was distinctly seen by the discoloration of the leaves, which commenced at the basis of each leaf and extended towards the apex, the lively green colour being changed into a dirty yellow. After some time this

coloration was followed by a dark blue one, commencing at the base of each leaf and extending towards the apex, but never quite reaching the latter except in the lower leaves. When the change in colour had begun to show itself in the upper tenderer leaves, the whole showed symptoms of fading, all further power of absorbing the acid liquid seemed to be lost, and the sprigs were then at once immersed in hot spirit of wine. After remaining in the spirit until the chlorophyll was removed, the part of each leaf which had undergone a change appeared blue, whilst the part into which the acid had not penetrated appeared almost colourless. In this case it is probable that the indican was decomposed not so much in consequence of the loss of vitality in the cells, as by the direct action of the acid. In some of the leaves there was another distinct blue coloration towards the apex, in the part to which the acid had not penetrated, separated from the blue part at the base by a white zone. This second coloration may be attributed to the loss of vitality in that part of the leaf.

All these experiments must be made with plants whilst in a state of vigorous growth. If made when the season is advanced (that is, after the flowers have begun to appear), the leaves, though apparently unchanged, show only traces of blue colour after treatment and subsequent immersion in hot alcohol. This shows either that the indican has disappeared, being applied to other purposes in the economy of the plant, or that it has undergone the peculiar molecular change before referred to, into a substance which no longer yields indigo-blue by decomposition, but indirubine and other products. The latter is the more probable way of accounting for the difference; for the parts of the leaf which have been accidentally injured by the bites of insects or from other causes at the later stage of the plant's development become red, not blue, as at the earlier stages.

This red colour disappears on immersion of the leaves in hot alcohol, the indirubine, to which it is probably due, being more soluble in that menstruum than indigo-blue.

The leaves of *Polygonum tinctorium* in which the blue colour has been developed by any of the means described, exhibit even to the naked eye, and still more distinctly when examined under the microscope, certain appearances which are not without some interest.

1. The colouring-matter seems to be confined to the parenchyma of the leaf. The stem and its fibrous ramifications in the leaf are free from it, so that in the coloured leaf the vessels may be distinctly traced as white veins on a blue ground. Even the cells of the parenchyma adjacent to the vessels are much less coloured than those a little further off, which produces the effect of a gradual shading of colour from the white of the vessels to the dark blue of the remoter cells. The cells of the leaf-cuticle are also free from colouring-matter.

2. The younger leaves at the summit of each branch generally show a more intense colour than the older ones near the base. Each leaf probably contains the same amount of colouring-matter; but in the lower leaves it is more widely distributed.

3. The intense and apparently uniform coloration of some of the leaves might lead to the conclusion that the cellular tissue is itself dyed blue—which would not seem improbable, considering the affinity which indigo-blue shows for cellulose, as seen in the blue dyeing of cotton fabrics. On examining the leaf-cells under the microscope, this is found, however, not to be the case. The colouring-matter is discovered within the cells of the parenchyma in the shape of separate dots and parcels of various sizes, and apparently in an amorphous state, the cell-wall being quite colourless. These dots and parcels being very numerous,

produce, when seen in the mass, a uniform blue coloration, more or less intense. The darker colour of some leaves is simply the effect of a greater crowding of the blue particles in each individual cell, the cells of the paler leaves containing fewer of these little masses, sometimes hardly any.

Mr. Charles Bailey, to whom I gave some specimens of leaves of *Polygonum tinctorium* coloured blue, had the kindness, at my request, to submit them to microscopic examination, and gave the following as his opinion thereon :—

"The colouring-matter left in these specimens would seem to be what Nägeli terms 'crystalloids;' and, with one exception, these bodies are, as far as my examination has gone, confined to the interior of the cells of the parenchyma. I do not see the least trace of any of this colouring-matter occurring in the intercellular spaces.. The only part of the tissue where I find it, other than the parenchyma, is in the cells of the stomata ; but it occurs nowhere else in the cuticle."

Bletia Tankervilliæ.

The occurrence of a blue colouring-matter in this and other plants belonging to the Orchidaceæ, such as *Calanthe veratrifolia*, was first noticed by Clamor-Marquart* and by the late Dr. Crace Calvert†. The attention of these observers was directed to these plants by seeing a blue coloration appearing in the white petals of the flowers on their beginning to fade; and they found the blue colour to be due to indigo. In accordance with the views then prevailing, they assumed the preexistence of the colouring-matter, either as indigo-blue or as its hydride, in the tissues of these plants. It is easy to see, however, on reading the accounts of their experiments, that the colouring-matter was really formed during the processes

* Buchner, Repertorium f. die Pharmacie, B. lvii S. i.

† Journal de Pharmacie, t. vi. p. 198.

employed for its extraction; and it seemed to me, therefore, highly probable that the plant would be found to contain some glucoside similar to indican.

Bletia Tankervilliæ is not difficult to procure, being frequently grown for the sake of its handsome brown and white flowers and its general beauty. The leaves of the plant having been cut in pieces and ground with water between two stones to a pulp, which is strained through calico, yield a green muddy liquid which, heated to near the boiling-point, gives a thick green coagulum. The liquid filtered from this coagulum is clear, of a deep yellow colour, with a slight acid reaction and an acrid taste. When this liquid is mixed with sulphuric or hydrochloric acid and left to stand, it deposits dark-coloured flocks consisting of indigo-blue mixed with a substance which imparts a purple colour to boiling alcohol, probably indirubine. The presence of glucose may be detected in the filtrate by the usual test, while the same test applied before the addition of acid shows no indication of its presence. It may hence be inferred that the liquid contains in solution a glucoside similar to, if not identical with, indican. It undergoes, like indican, a complete change when submitted to the action of alkalies; for if its watery solution be mixed with caustic alkali and boiled, then with an excess of sulphuric acid and again boiled, it deposits brown flakes, which are found to contain but little indigo-blue, while in the filtrate only a trace of glucose can be detected. The same change takes place gradually when the watery solution is left to stand for several days at the ordinary temperature.

The gradual formation of indigo-blue in the leaves of *Bletia Tankervilliæ* may easily be traced in the same way as with those of *Polygonum tinctorium*, by placing the lower ends, immediately after cutting, in dilute hydrochloric acid and leaving them freely exposed for a few days. The acid,

as it ascends, causes a dark discoloration ; and the part discoloured, after immersion in boiling alcohol to remove the chlorophyll, appears blue.

Indigofera tinctoria.

It would be a matter of some interest to ascertain in what state the colouring-matter exists in this the most important of all the plants yielding indigo. From what I have said, however, it will be apparent that, in order to arrive at a certain conclusion, it would be necessary to work with fresh leaves; for if they contain a glucoside resembling indican, this would, in a very short time, undergo complete decomposition. I obtained some seeds of *Indigofera tinctoria* from Messrs. Vilmorin, Andrieux & Co., and treated them in accordance with the directions they kindly gave me. The seeds germinated and the young plants lived for some time in a hothouse; but, unfortunately, they attained no great size, and soon decayed and died, so that I was unable to obtain a quantity of leaves sufficient for examination.

Mr. P. Michéa, an intelligent indigo-planter, with whom I have been in correspondance, gives me, however, some interesting information relating to this part of the subject. Mr. Michéa writes to me as follows :—"It was my finding in the *Indigoferæ* of India (in the wild plant which grows at many places in the three Presidencies as well as in the cultivated species of Bengal, the North West, and Madras) a glucoside substance perfectly similar to the indican of the *Isatis tinctoria* in all its properties, which made me declare that the colouring-matter of the Indian *Indigoferæ* was due to indican."

The experiments just described lead to the onclusion that in all the indigo-yielding plants hitherto examined, the colouring-matter is derived from a glucoside which

splits up with great ease into indigo-blue and glucose, and that this glucoside is probably, in all cases, the same, and identical with the indican of *Isatis tinctoria*.

Various other plants have been supposed to yield indigo-blue. Of these I have examined the following:—

Galega officinalis.
Hedysarum Onobrychis (Sainfoin).
Polygonum Fagopyrum (Buckwheat).
Polygonum Persicaria.
Rhinanthus Crista-galli.
Sophora japonica.
Spilanthes oleracea.

The leaves of these plants, when treated in the manner above described, yielded no trace of any substance resembling indican, and showed no indications of containing any blue colouring-matter like indigo.

XXIX. *On Siliceous Fossilization.* By J. B. Hannay, F.R.S.E., Assistant Lecturer on Chemistry in the Owens College. Communicated by Professor Roscoe, F.R.S. &c.

Read March 19th, 1878.

My friend Professor Young having mentioned to me that certain questions connected with fossilization were in a very undecided state, and having expressed his opinion

that a chemical examination was really what was required to throw light on the question, I obtained from him some siliceous fossils in which he was interested, and of which he and Mr. J. Young, F.G.S., have published an account*, and proceeded to make an examination of them by chemical means. The rods were samples of *Hyalonema Smithii*; and as they were in the fossil state siliceous, and probably had been so in the original state, I took for comparison in my examination of them some rods of a recent siliceous sponge. On examination of the fossil rods it was seen that they might roughly be divided into two classes:—I. rods partly transparent or opalescent, and containing some chemically combined water; and II. rods opaque and apparently consisting of anhydrous crystalline silica,—both classes, however, containing many samples stained brown by ferric oxide. Thus I generally experimented with three different kinds, which shall be referred to as

I. Recent, quite transparent.

II. Fossil, opalescent.

III. Fossil, opaque.

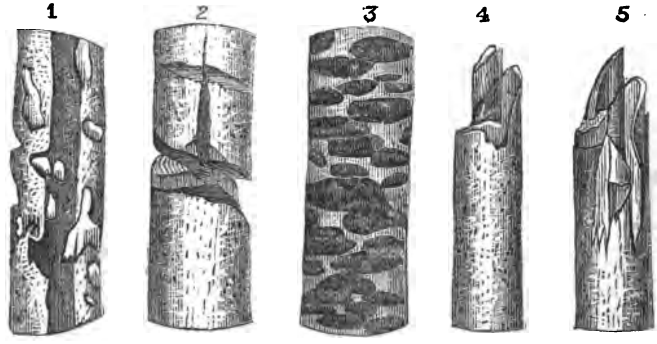
To find the general differences displayed by the three kinds, several experiments with various chemicals were first performed, placing all the three kinds in the same vessel under the same circumstances. On placing the three kinds in hydrofluoric acid, it was seen that the recent rod was dissolved in about thirty minutes, the opalescent in about two hours, and the opaque rod only partially dissolved after four hours' immersion in the acid. Sometimes the opaque rod dissolved irregularly, leaving protuberances, and sometimes leaving a spongy substance covering the still undissolved interior. On heating the three kinds in a four-

* Annals of Nat. Hist. vol. xx. No. 119, p. 425.

per-cent. solution of caustic soda in a sealed tube to a temperature of 110° C., so that they were under pressure, the recent rods dissolved in about ten minutes; No. II. was nearly all dissolved in thirty minutes, leaving only a thin core of anhydrous silica; while after two hours No. III. was only irregularly eroded and in some places seemed very little affected. On heating the three specimens to 100° with water for two days, very little change was observed; in fact water at the boiling-point was found to be without practical effect; but when heated to 150° C. in a sealed tube, so that it was under a pressure of several atmospheres, the water had the effect of making No. II. much more transparent. It seemed, then, that at this high pressure the substance took up water, and so was rendered more similar to the recent sponge rods. I found, however, that if a substance like ether, which boils at a much lower temperature, were introduced into the tube above the water, so that the pressure would increase much more rapidly than with water, the rods might be hydrated at a lower temperature. On heating the rods by themselves, No. I. gave off water and became friable, sometimes exfoliating, No. II. also gave off water and became opaque like No. III., and at the same time friable, while No. III. underwent no change.

The general conclusions to be drawn from these experiments are that the principal difference between the three kinds of rods lies in the relative proportions of water they contain, and that the larger the proportion of water contained in any sample the more transparent is it. From even a cursory examination of the rods it may be seen that as the proportion of water decreases the original features of the rods become more and more obliterated, so that a totally dehydrated rod is reduced to a mass of crystalline inorganic silica. It will be seen that an examination of the state of

combination of the water in the different rods would be likely to throw light on their constitution; but, owing to the small quantity of rods at my disposal, the method for thus examining substances, which I have published elsewhere*, was in this case inapplicable, an optical examination being the method which promised to yield the best results. Heating the rods with caustic-soda solution did not give results throwing any light on their structure, the general effect of this reagent being to erode them, as shown in fig. 1. An attempt was made to gain a knowledge of

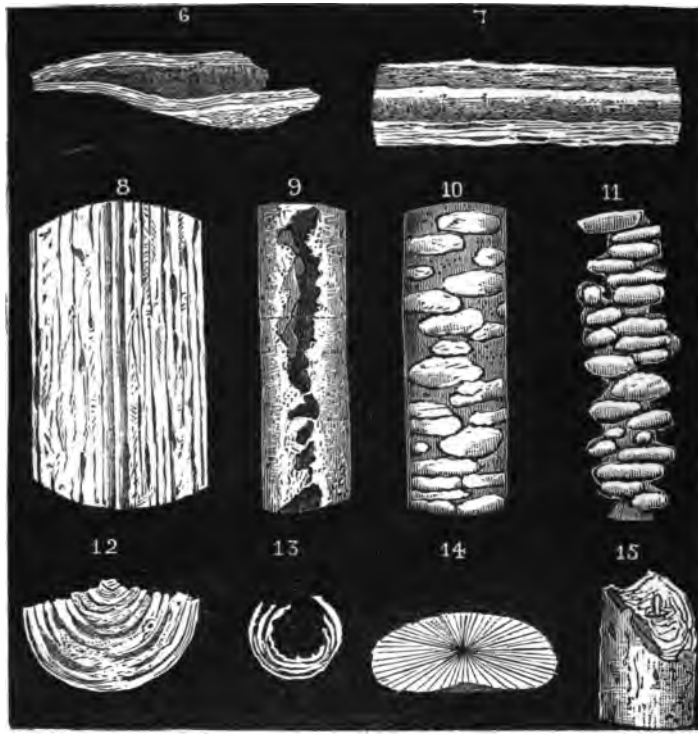


the structure of the rods by imbedding a specimen of No. II. in Canada balsam so as to leave only one half exposed, and then to etch away this upper portion by means of hydrofluoric acid. This method succeeded pretty well, giving a sectional view of the internal structure of the rod, as shown in fig. 7. The layers are seen by this method to be pretty clearly marked; but the etching is very irregular.

The next method tried was rendering the opalescent nodules transparent by means of high-pressure steam or, rather, water, as the substance was always surrounded by

* "Examination of Substances by the Time Method," Chem. Soc. Journ. No. clxxvii.

water, even when the temperature rose to 150° C. A fractured rod (fig. 2), thus rendered transparent, shows on the fracture plain markings of the layers composing the



rod cylindrically arranged. It was found that when a rod was heated with water in a tube to 150° for five or six hours, the side of the rod next the glass became dissolved, this action seeming to be due to the solvent action of the soda of the glass, as water at that temperature decomposed the glass, removing the alkali. Fig. 9 shows a rod so treated; and fig. 13 shows a section of that rod through the dotted line in fig. 9. This action clearly shows at least four layers in the rod. Fig. 15 shows the action of

water containing a small quantity of alkali at 150° on a rod the end of which has dissolved to a considerable extent, leaving a hard core of anhydrous silica which, as my former experiments showed, is unacted on by dilute caustic soda.

It occurred to me that if a section were first prepared by grinding or splitting, and this section hydrated as much as possible and then acted upon by the hydrofluoric acid, it might show the state of the rod, as the hydration of the silica would restore the rod to its original state, and the etching action of the acid would show what that state was.

The experiment was carried on in this way. A rod was split by mechanical means and placed in a thick short tube sealed at one end along with some water. A thin tube much smaller than the other was filled with ammonium fluoride solution, sealed and placed in beside the nodule; and the larger tube was then sealed off and heated in the air-bath to 150° C. In rather less than half an hour the ammonium fluoride dissolved its way through the thin tube, and, escaping into the larger tube, etched the split rod. By these means the silica of the rod was first hydrated, and whilst in this state was etched by the fluorine. My reason for wishing the etching to go on after the hydration and without removing the rod or cooling it, was that when the rod is removed from the superheated water it begins immediately to lose its water; that is, the hydration is only temporary. By the above method the rod was not allowed to become dehydrated, but was etched just as the hydration was going on.

Figs. 8 and 12 are specimens obtained by this method; and they show the layer formation to perfection, as many as fifteen layers being shown by one specimen.

Fig. 12 is the end of fig. 8; and it shows the concentricity of the layers very plainly.

Fig. 6 is a small splinter, the centre of which was in a higher state of hydration than the exterior, so that, when treated with hydrofluoric acid, it left a hollow interior. The portion left shows the layers also.

I noticed, when rendering some of the opalescent rods transparent, that they did not all show the same equally distributed transparency; but when viewed by transmitted light, dark rounded patches of opaque matter appeared irregularly throughout the transparent portions, as shown by fig. 3. When viewed by reflected light, they showed themselves as white patches upon the opalescent ground, as in fig. 10. It was found that by heating these rods in dilute caustic-soda solution (about 2 per cent.) in sealed tubes to 130° C., the transparent portion could be dissolved away, leaving rounded nodules held together by a portion of the opalescent substance, which, viewed by reflected light, had the appearance given in fig. 11.

Dr. Young has figured in his paper rods which are made up of nodules, and which present very much the same appearance as shown in the above figure if the whole of the opalescent portion were dissolved away; and it would appear as though they had been formed in somewhat the same way—that is, by the solution of the more highly hydrated portions. The question is, how are the rounded nodules formed in the rod? One of the largest I could find was carefully split, and its structure examined; and it was seen, when viewed under the microscope by reflected light, to have the appearance presented in fig. 14; that is, it had the character of a radiating nodule of crystalline silica.

Now it appears from the above evidence that in this case of siliceous fossilization the process goes on in something like the following way. The clear transparent rods first lose a portion of their water, becoming transformed

into the opalescent or semigelatinous-looking form No. II.; and in so doing they begin to lose some of their internal structure; that is, the layers adhere more closely, and the rods become brittle and lose the glass-like flexibility they originally possessed. At this point any further loss of water causes crystallization to set in, the silica not being able with a lower proportion of water to remain in the "organic" state; and if this crystallization sets in at a great many points, the nodule soon becomes an opaque mass of crystalline silica as in variety No. III.; but if crystallization sets in at comparatively few places and spreads slowly out, it forms the kind of nodules drawn into fig. 10. If at this point any solvent agent comes into contact with the rod, it dissolves away the non-crystalline portion, leaving nodules as shown by Dr. Young and in fig. 11; but if no solvent action goes on, the rod ultimately becomes transformed into variety III., which is crystalline silica.

The transformation from "organic" to "inorganic" silica is one of arrangement; and as the "organic" arrangement requires a certain amount of water, whenever the proportion falls below this a simpler arrangement sets in, and we have crystalline silica. That the rods of *Hyalonema Smithii* are the same in constitution at least as those from modern sponges is, I think, proved from these experiments; and to show a further analogy, I have drawn a fractured specimen of such in figs. 4 and 5, the similarity of which is very striking.

XXX. *On a New Calorimeter.* By J. B. HANNAY, F.R.S.E., Assistant Lecturer on Chemistry, Owens College.

Read March 19th, 1878.

HAVING been desirous some time since of making a number of calorimetric determinations, I set about constructing an instrument for the work; but I found that working in the rapid irregular manner in which I was compelled to do by using only odd leisure moments, the difficulty of obtaining a perfectly constant temperature was very great; and as I have, in a great measure, overcome that difficulty, I think that an account of the instruments which I have constructed may be of some use to workers who may be placed in such a position as mine, and may desire to make measurements quickly and without waiting until the temperature is quite constant.

In the first place, it is evident that, as the external or accidental variation of temperature of the instrument is generally not more than 0·2 to 0·3 of a degree, if the internal variation be made greater in one case than another the error arising from accidental variation will be smaller. Now, if we use a liquid body to expand as an indicator, we must, if we want a large rise of temperature, use a small quantity of the liquid or a large quantity of the body experimented with; either of these conditions, however, involves difficulties of a kind nearly as bad as the temperature-variations. But if we use a gas as our index—that is to say, if we use an air-vessel into the middle of which we plunge the substance whose specific heat we wish to determine, and have the vessel surrounded

by the usual non-conducting material, we can have a large rise of temperature in a considerable space of matter. Of course at the present time, when the specific heat of substances such as silver, copper, and iron have been very accurately determined, we do not need to have a water or absolute calorimeter, but only an instrument which will accurately register the rise of temperature, and the value of whose graduations may be determined by standard metals; so that although we do not know the absolute calorimetric value of the heat communicated to the air and glass of such an instrument, yet, if its indications are proportional to the amount of heat communicated, we have all that is required to form a relative calorimeter.

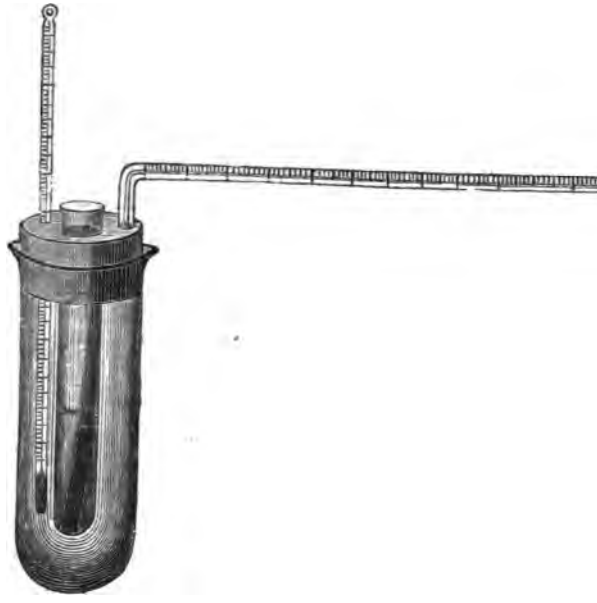
Then as to the construction of such an instrument. It was found that the use of ground joints in the instrument might be entirely avoided, as a red india-rubber stopper can be adjusted with nearly as much accuracy as a ground joint.

The apparatus shown below as fig. 1 is the first form of the calorimeter I constructed. A ring was etched near the top of the wide outer tube, which was then filled with water and the india-rubber stopper pressed in till it was level with the ring (the excess of water escaping by the index-tube) and the whole weighed. This was repeated several times; and it was found that (provided the temperature were constant) the weights differed only by two or three centigrammes at the most. If, however, the stopper were permanently fixed in, and the index-tube and thermometer and interior tube pushed into their proper marks afterwards, the weights differed only by a few milligrammes. It was thus seen that the volume of gas in the apparatus could be adjusted with as great accuracy as by ground stoppers and with less chance of leakage.

The apparatus consists, then, of a wide outer tube having

a horizontal ring etched round it (in case it may be necessary to replace the stopper) and fitted with an india-rubber stopper, through which the thermometer, reception-

Fig. 1.—The New Calorimeter.



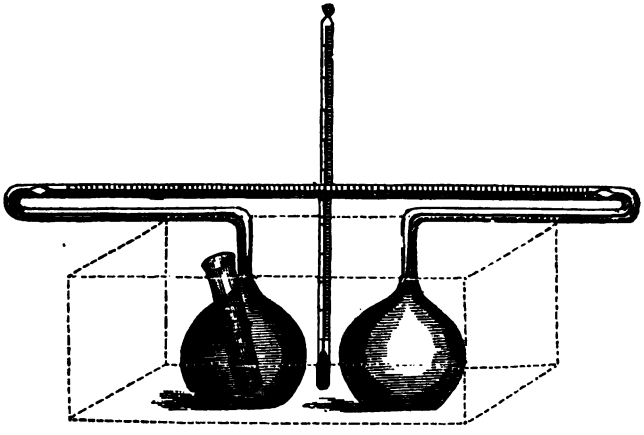
tube, and index are inserted. The reception-tube may also have a ring etched round it and be filled with water or other liquid to that mark in order to cause any non-conducting body to give up its heat quickly to the glass; but I generally prefer to use the apparatus dry and stop the end of the reception-tube with cork in order to prevent convection-currents. When this latter method is used, the apparatus is always ready for use and requires no other preparation than placing the index in position, which is managed as follows :—With regard to the substance of

the index, I have found mercury not sufficiently mobile; so I have used instead a coloured aqueous solution preferably containing a little glycerine. The calorimeter is firmly clamped and enclosed in wool, and a small drop of coloured liquid is made to touch the end of the index-tube, when it is at once drawn. The number on the scale of the thermometer level with the stopper is read off, and the thermometer, which has been slightly greased, is pulled up (screwing it from side to side to make it rise easily) till the coloured liquid is drawn up to zero on the scale; then the number on the thermometer-stem level with the stopper is again read off. We have then the volume of the scale-tube in terms of degrees on the thermometer; and so, whenever a calorimetric determination is about to be made, the thermometer is pushed down that amount beyond its standard level, and just before a determination is made the index is introduced and sucked in (by means of the thermometer) to zero. It will thus be seen that the calorimeter is open to the air and in a state of equilibrium until the experiment is about to be made; and as the index is adjusted by the above-mentioned simple contrivance, no special parts are required for the purpose. As this calorimeter can be made and used by any student, I have found it extremely useful in inculcating the principles of thermal chemistry.

It naturally occurred to me that by connecting the end of the index-tube with another air-vessel, which would be kept in the same conditions as the calorimeter-vessel, we should have an instrument in which the external or accidental variations affecting both sides of the system would in no way affect the equilibrium, and the index would only be moved by any difference affecting only one vessel—or, in other words, we should have a differential calorimeter. Fig. 2 shows the construction of such an instrument. Two

glass bulbs (one holding an interior tube for the reception of the substances, and the other a little smaller in diameter in order that its cubic contents may be about the same)

Fig. 2.—The Differential Calorimeter.



are fitted with an index-tube of small bore, as shown in the figure, which is graduated between the two small cavities on the upper portion of the tube, the cavities being there to prevent the index being accidentally drawn into either of the bulbs. The two bulbs are placed in a box (indicated by dotted lines) surrounded by non-conducting material, and the instrument is ready at any time for use. A thermometer is placed between the bulbs in order that the temperature may be noted at which the experiment was tried. In the instrument No. 1 the indications are nearly proportional to the specific heat of the substance; but in the second or differential calorimeter a curve should be drawn of the values for known metals, just as is done for the spectroscopic scale, and the specific heats of the bodies reduced by reference to the curve. To show the accuracy

of the instruments, I give here some specific heats taken at random from many I have done.

First by No. 1 Calorimeter:—

Substance.	Found.	Correct numbers.
Arsenic	·0820	·0814
Tin	.0575	·05623
Platinum	·0339	·0335
Bismuth	·0315	·03084

I should say that I took as my standards to draw the reference-curve the following metals:—

Lead	·03065
Silver	·05701
Zinc	·09555
Iron	·11380
Magnesium	·24990

The differential calorimeter gave:—

Substance.	Found.	Correct numbers.
Arsenic	·0818	·0814
Tin	·0565	·05623
Platinum	·0338	·0335
Bismuth	·0309	·03084

In conclusion, I would say that the form of air-calorimeter I have given as No. 1 is very well adapted for teaching, as it enables students to construct for themselves instruments of sufficient exactitude to yield numbers very closely agreeing with standard numbers; while for calorimetric investigations, where freedom from all error is the first requisite, the differential calorimeter is very well adapted. The latter may also be made with india-rubber joints; but, as there is pressure, I prefer to have the joints hermetically sealed. In determining the heat of combination by these instruments, the same relative method is used as for specific heats. It will be obvious that one

great advantage in these instruments is the very small amount of substance required to give the necessary deflection. I have found in practice that 0·5 to 2 grammes is quite sufficient, the amount, of course, varying with the size of the instrument.

XXXI. *On Aurin.*

By R. S. DALE, B.A., and C. SCHORLEMMER, F.R.S.

Read April 2nd, 1878.

IN a paper read before this Society on 31st October, 1871, we gave a short account of this colouring-matter, which was discovered by Kolbe and Schmitt in 1861. We showed that the commercial product, which is called aurin or corallin, and is obtained by heating phenol with sulphuric acid and oxalic acid, is a mixture of different bodies, from which we isolated a colouring-matter which crystallized exceedingly well and for which we retained the name of aurin*. The analysis of this body gave numbers agreeing with the formula $C_{20}H_{14}O_3$, from which we concluded that it was produced by the action of nascent carbonic oxide on phenol :—

$$3C_6H_6O + 2CO = C_{20}H_{14}O_3 + 2H_2O.$$

We further showed that aurin, when treated in an alkaline or acid solution with zinc-dust, combines, like most

* A full account of this investigation is found in Journ. Chem. Soc. (2) xi. 434.

other organic colouring-matters, with hydrogen, yielding leucaurin ($C_{20}H_{16}O_3$), which readily crystallizes in large colourless prisms, whilst on heating aurin with ammonia under pressure a new red colouring-matter is formed which is called red aurin or peonin.

The further investigation of this body showed that it contained nitrogen, which, on heating with soda-lime, was evolved in some cases completely as ammonia, while in others, where the action of the ammonia went on longer, it was given off, at least partly, in the form of aniline. We then found that, on continuing the heating, the red colour of the liquid became paler, and at last a yellowish solution was obtained, from which water precipitated a white crystalline body possessing all the characteristic properties of rosaniline*. The formation of this compound might be explained by the following equation:—

$$C_{20}H_{14}O_3 + 3NH_3 = C_{20}H_{17}N_3 + 3H_2O.$$

But Hofmann, who has most carefully examined rosaniline as well as a great number of its derivatives, found that the formula of this base is $C_{20}H_{19}O_3$; and this result has been confirmed by Caro and Gräbe†, who showed that rosolic acid, which is obtained from rosaniline by a reaction which may be shortly stated to be the inverse of that above given, has the composition $C_{20}H_{16}O_3$, whilst its properties are in almost all respects so similar to aurin that some chemists, including ourselves, have been inclined to regard them as identical.

In order to clear up these points, it was necessary to prepare a considerable quantity of pure aurin. We have already shown, in our memoir read before the Chemical Society‡, that a pure compound is readily obtained by

* Journ. Chem. Soc. (2) xi. 434. † Liebig's Ann. clxxix. 148.

‡ Journ. Chem. Soc. 1877, ii. 121.

heating pure phenol with oxalic and sulphuric acids for a few days to about 105° C. Although a good product is thus obtained, the yield is but small, while on heating the mixture longer, or to a higher temperature, by-products are formed. Some chemists have stated that the formation of the latter cannot be avoided, and that it is very difficult to obtain thus a pure aurin. We believe that we are able to explain these different results. The formation of a pure compound depends much on the proportions and the manner in which phenol and the acids are mixed. As, however, the preparation of pure aurin by this method is a very tedious process, we endeavoured to obtain it from the commercial product by the method which we have described in our first paper, and purifying it by repeated recrystallization from alcohol, the crystals separating out first being used for further purification, because we found that the purer aurin is, the less soluble it becomes, its solubility being increased by the admixtures which are present in the crude product.

Another method for purification which we employed consisted in washing good commercial aurin with cold alcohol until only a comparatively small residue was left, which was then treated as above stated.

The different crops of crystals were analyzed, and the purification continued until the product did not change its composition any more. A great number of analyses were thus required; and we have great pleasure in expressing our best thanks to Mr. L. T. O'Shea, who performed them with great care.

The results did not, however, agree with our former; the product, having a constant composition, and crystallizing exceedingly well, gave numbers agreeing with the formula $C_{19}H_{14}O_3$. We therefore again prepared aurin from pure phenol, and crystallized it repeatedly from al-

cohol, until its composition became constant, and found it to have the same formula.

Aurin therefore appears to be the lower homologue of rosolic acid; like the latter, it readily forms a tetrabromo compound, which we obtained pure by the following process:—Pure aurin was dissolved in hot glacial acetic acid, and this solution poured into a mixture of an excess of bromine and acetic acid. The liquid remained perfectly clear; but on standing for a few days, the tetrabromaurin crystallized out almost completely. It forms small brownish-green crystals with a metallic lustre, which dissolve in alkalies with a purple colour. The determination of the bromine gave numbers agreeing closely with those required for the formula $C_{19}H_{10}Br_4O_3$.

Whilst this investigation was going on, a new light was thrown on the subject by the researches of E. and O. Fischer. In order to elucidate the constitution of rosaniline, these chemists endeavoured to convert rosaniline into the hydrocarbon from which it is derived. In this they did not succeed; but they were able to transform leucaniline ($C_{20}H_{21}N_3$), which stands to rosaniline in the same relation as leucaurin to aurin, into a hydrocarbon $C_{20}H_{18}$, which is the mother substance of this base, and from which it follows that the hydrocarbon corresponding to rosaniline has the composition $C_{20}H_{16}$, which also confirms the correctness of Hofmann's formula for rosaniline. The leucaniline which they employed was obtained from commercial rosaniline; but Rosenstiehl has shown that this body, which is prepared by oxidizing a mixture of aniline, solid paratoluidine, and liquid pseudotoluidine, contains several, probably isomeric, bases. E. and O. Fischer therefore, in continuing their research*, prepared one of them in the pure state by using a mixture of pure aniline

* Ber. deutsch. chem. Ges. xi. 195.

and pure paratoluidine. The product was converted into the leuco compound, from which, to their surprise, they obtained, not as before, the hydrocarbon $C_{20}H_{18}$, but one having the composition $C_{19}H_{16}$, or the lower homologue of the former. They found the new hydrocarbon to be identical with one already known, which was discovered by Kekulé and Franchimont, and called triphenylmethane, $CH(C_6H_5)_3$, as it is marsh-gas or methane in which three atoms of hydrogen are replaced by three phenyl groups. Hemilian, who afterwards obtained the same hydrocarbon by a different method, examined it more fully, and found, amongst other results, that concentrated nitric acid converts it into the trinitro compound $C_{19}H_{13}(NO_2)_3$. E. and O. Fischer prepared the same body, which, by the action of nascent hydrogen, was converted into a base having all the characteristic properties of leucaniline. This was heated with arsenic acid in order to abstract hydrogen by oxidation; and the rosaniline, which formed the starting-point of their last investigation, was obtained again.

From these results it follows that commercial rosaniline consists principally of the base $C_{20}H_{19}N_3$, and is probably a derivative of pseudotoluidine, while the rosaniline from paratoluidine, and which has therefore been called para-rosaniline, is its lower homologue, having the formula $C_{19}H_{17}N_3$. The rosaniline which we obtained from aurin is undoubtedly identical with the latter*, its formation being readily explained by the following equation:—

$$C_{19}H_{14}O_3 + 3NH_3 = C_{19}H_{17}N_3 + 3H_2O.$$

The question which has now to be solved is how to explain the formation of aurin. We have already made ex-

* Since this was written, E. and O. Fischer prepared some pure aurin according to our directions, and converted it into a leucorosaniline, from which they also obtained triphenylmethane (Ber. deutsch. chem. Ges. xi. 473).

periments in this direction and obtained interesting results, which, however, we have not yet sufficiently worked out to publish them.

Another question to be elucidated is, what is the chemical constitution of aurin? According to E. and O. Fischer, that of pararosaniline is $C_{14}H_{11}(NH_2)$, from which it follows that aurin is $C_{14}H_{11}(OH)_3$. Already, in our first research, we obtained results which seem to confirm this view. On heating aurin with acetyl chloride, we obtained a colourless crystalline compound which, on analysis, gave results nearly agreeing with the composition of triacetyl aurin, $C_{19}H_{11}(OC_2H_3O)_3$; and on using benzoyl chloride a similar benzoyl compound was obtained. The analytical numbers, however, did not sufficiently agree with those required by theory; and therefore we did not publish them; and as the purification of these compounds was found to be beset with several difficulties, we did not then examine them further, because we believed the study of red aurin would more readily throw light on the subject. We have now prepared them again from pure aurin, and hope to be soon able to settle these points.

XXXII. *The Origin of some Ores of Copper.*—Part II.

By CHARLES A. BURGHARDT, Ph.D., The Owens College.

Read April 2nd, 1878.

Atacamite.

THIS interesting and beautiful mineral occurs generally in

radiating foliating masses, or in bundles of acicular crystals which are characterized by a fine vertical striation upon their vertical faces. Globular, reniform, and stalactitic atacamite is also known. Thus it is safe to infer that in a great majority of cases the mineral has been formed in some way from a solution of copper-salt, and not by igneous action. It is also a well-known fact that atacamite crusts are occasionally found resting upon clefts in the lava of some volcanoes, being formed by the action of hydrochloric acid gas evolved by the volcano upon copper or a compound of that metal; but these crusts are insignificant in quantity and comparatively scarce. In Chili, Bolivia, and the west coast of Africa very large quantities of atacamite are obtained as a commercial product; it therefore becomes interesting to the mineralogist to ascertain under what conditions these formations arose; and in order to do this, a thorough chemical examination of the mineral itself is necessary. Atacamite is known to occur in nature in three states of hydration; and as an example of the first I may mention a specimen from Algodon Bay, Bolivia, analyzed by Bibra (Jahresbericht, 1858, p. 740), which had the following composition, viz.:—

	per cent.
Cu	59·25
Cl	16·11
O	12·51
H_2O	12·13
	100·00

the formula corresponding with the above analysis being $Cu_4O_3Cl_2+3H_2O$.

The atacamite of Copiapo, Chili, as analyzed by Field (Journ. Chem. Soc. [7] iii. 193) has the following composition, viz.:—

	per cent.
Cu	56·38
Cl	14·95
O	10·78
H_2O	17·89
	100·00

the formula for this being $Cu_8O_6Cl_4 + 9H_2O$, or $(Cu_4O_3Cl_2)_2 + 9H_2O$.

The third state of hydration is represented in the atacamite from Botallack, Cornwall, analyzed by Church (Journ. Chem. Soc. [2] iii. 212), and that from Tocopilla, near Cobija, Bolivia, analyzed by Berthier (Ann. des Mines [3] vii. 542), the composition being as follows :—

Botallack Atacamite.		*Tocopilla Atacamite.*	
Cu	52·90	Cu	53·26
Cl	14·76	Cl	14·92
O	10·19	O	9·37
H_2O	22·15	H_2O	22·45
	100·00		100·00

and the formula $Cu_4O_3Cl_2 + 6H_2O$.

Having given a general idea of the composition of the various atacamites, I will now point out how some of them can be prepared artificially. Field (Phil. Mag. [4] xxiv. 1862, p. 124) prepared the last-mentioned variety by adding a solution of calcium hypochlorite to an excess of cupric sulphate and boiling the mixture for some time, when an apple-green precipitate was observed which proved, on analysis, to be identical with Berthier's atacamite from Cobija. If the ebullition were not sufficiently kept up, the compound was found to contain only four atoms of water. Proust prepared a green compound (which, no doubt, was atacamite, although its exact composition is not given) by exposing moist cuprous chloride to the air, also by digesting a solution of cupric chloride with cupric

oxide. In a former communication to this Society (Proc. Lit. and Phil. Soc. xvii. 1877–78, pp. 27–36) I mentioned that on heating some crystals of cuprous chloride in water in a sealed tube, at a temperature ranging from 160°–180° C., minute green spots of a substance resembling atacamite were observed upon the sides of the tube. Further, on heating cuprous oxide and a strong solution of chloride of sodium together in a sealed tube, at 150°–180°, chalcotrichite was formed, and also a green substance resembling atacamite.

Wishing to ascertain whether atacamite could be prepared in larger quantity (in accordance with my supposition that all the copper-ores are products of the decomposition of cuprous oxide, the latter substance being a secondary product of the oxidation of the metal itself), I made numerous experiments, mostly consisting in the heating of cuprous oxide with sodium-chloride solution in sealed tubes, and obtained excellent results by simply covering a quantity of cuprous oxide with a concentrated solution of sodium chloride, exposing this mixture to the air at the ordinary temperature, and stirring it up occasionally. By proceeding in this way, a very concentrated solution of cuprous chloride in sodium chloride can be obtained. After standing a few days, this solution begins to decompose, a green insoluble substance separating out continually, which can be easily filtered off from the liquid portion and well washed in cold distilled water in order to free it from all traces of cuprous chloride and sodium chloride. The substance thus obtained was carefully dried over calcium chloride until its weight was constant, and portions weighed out and analyzed quantitatively, with the following result, viz. :—

	per cent.
Cu	56·25
Cl	14·29
O	10·95
H_2O	18·51
	100·00

It will readily be seen that the above analysis agrees very closely with the one made by Field already referred to; whence it follows that the substance prepared by myself is identical in composition with the atacamite of Copiapo, Chili. The rationale of the formation is briefly this:—The cuprous chloride held in solution by the sodium chloride takes up oxygen from the air very rapidly, giving rise to the formation of cupric oxychloride and cupric chloride (the latter soluble in water), both eventually becoming hydrated." The following equation expresses the first stage of the reaction, viz.:—

$$3Cu_2Cl_2 + O_3 = CuCl_2, 3CuO + 2CuCl_2.$$

Nearly all specimens of atacamite are intimately associated with cuprite; so that it appears highly probable that the former mineral has been obtained from the latter in the manner stated, more especially as atacamite is found in veins of other copper-ores associated with quartz, chalcedony, &c. in diorite and syenite. Again, at Sierra de Lembe, near Ambriz, on the west coast of Africa, there is a large occurrence of atacamite in the malachite deposits, the "gangue mineral" being quartz. Debray (Zeitsch. f. Chem. 1867, p. 276) prepared crystallized atacamite by heating together basic cupric nitrate and a concentrated sodium-chloride solution for several hours at 200° C., also by heating copper-ammonium sulphate with a strong sodium-chloride solution at 100° C. Up to the present time I have not succeeded in preparing the mineral in a

crystalline state. A curious fact I may mention, viz. that natural cupric oxychloride should increase in hydration by three atoms of water.

XXXIII. *On some Colorimetrical Experiments.*

By JAMES BOTTOMLEY, B.A., D.Sc.

Read before the Physical and Mathematical Section, April 23rd, 1878.

IF a white disk be sunk in a coloured solution, the intensity of the colour perceived will be a function of the depth of the disk and the quantity of colouring-matter present. If, in any set of experiments, we make the intensity of colour constant, then the depth of the disk will be a function of the quantity of colouring-matter only. Occasionally in chemistry we have to deal with quantities so minute that we can no longer use the balance for their determination; and yet those small quantities, when they yield coloured solutions, are evidently comparable among themselves. In a short communication which I made to this Society (vol. xv. p. 63) I suggested that such a process as above indicated might be applied for the measuring of such traces, assuming that the depth of the disk would be inversely as the quantity of colouring-matter present. Some experiments were made on colouring-matter in solution. Glass cylinders were used about 10 inches high and 2 inches in diameter. To one side of each cylinder was

pasted a strip of paper marked with an arbitrary scale (each degree of the scale I afterwards found to be 27 millims.); the white surfaces used were crucible-lids; a piece of glass rod resting on one of the cylinders, having the string of the movable disk attached to it, served as a windlass for raising or depressing it. To avoid the use of many ciphers, I have taken ·0001 grm. as the unit of measurement. In the following experiments column A denotes the amount of the colouring-salt present, B the depth of the disk, and C the amount of the colouring-salt thence derived by calculation. Standard solution 35·2 of permanganate of potash in 500 cub. centims. of water (depth of disk 8·3):—

A	B	C
53	6·1	48
70	4·1	71

Experiments were made with still stronger solutions; but the differences were considerable. Standard solution 24 of permanganate of potash in 500 cub. c. of water (depth of disk 8·3) :—

A	B	C
36	5·4	37
48	3·6	55
60	2·8	71

Still stronger solutions were used with this standard; but the discrepancies between the true and the calculated quantities were considerable. Standard solution 5·8 in 500 cub. c. of water (length of column 8·3) :—

A	B	C
7·2	5·7	8·4

Some experiments were made with solutions of perman-

ganate of potash, using a shorter column for the standard solution. Standard solution 2·4 in 214·6 cub. c. of water (length of column 3·8) :—

A	B	C
3·6	2·2	4·1

An experiment was also tried with a still weaker solution. Standard solution 1·4 in 214·6 cub. c. of water (length of column 3·8) :—

A	B	C
2·4	2·6	2·1

Some experiments were also made by looking through the column of fluid at a white surface external to the cylinder. Standard solution 12 in 500 cub. c. of water (length of column 8·4) :—

A	B	C
14·4	6·5	15·5
28·8	3·1	32·5
16·8	5·3	19·0

I prepared a fresh standard solution of the permanganate and compared the last solution with it. The new standard was similar to the last in composition :—

A	B	C
16·8	5·7	17·6

Thus the result was somewhat more favourable with a newly prepared solution. I used the permanganate on account of its great tinctorial power; but it is a salt of considerable instability, especially in contact with organic matter. The water used in the experiments was obtained by distilling Manchester town's water from a copper still

lined with tin, and condensing in a tin condenser. Now, such water would not be wholly free from oxidizable matter; hence, in a little while, there will be a depreciation in the tint of the permanganate solution. If the tint be strong, the error arising from this cause may be neglected; but when the tint is feeble, the error is appreciable. If the depreciation in tint in the two solutions compared were proportional to the depth of tint in each, this source of error might be neglected; but, inasmuch as the same quantity of water has been used in making each solution, an equal amount of depreciation is introduced into each. Some of the foregoing experiments will be affected by this source of error. Small sources of error which may be neglected when we deal with ponderable quantities, become of sufficient magnitude to vitiate results when we deal with traces.

For experiments with a yellow solution I took a solution of bichromate of potash. Standard solution 10 in 500 cub. c. of water (depth 8·3) :—

A	B	C
15	5·4	15·4
20	4·4	18·8
30	3·5	23·7
40	2·5	34·0
50	1·7	48·8
60	1·5	55·3

Still stronger solutions of the bichromate were compared with this standard; but the discrepancies were considerable. Some of the results also given in the last table are not very good; this more especially applies to the third and fourth results. On another occasion I repeated some of these experiments with the following results. Standard solution same as last :—

A	B	C
30	3·0	27·6
40	3·2	37·7
50	1·6	51·8
60	1·4	59·3

Another series of experiments was made with the bichromate. Standard solution 50 in 500 cub. c. of water (length of column 8·3) :—

A	B	C
60	6·7	62
70	5·3	78
80	4·5	92
90	4·0	104
100	3·7	112
110	3·4	122
120	3·2	130
130	3·0	138
140	2·8	148
150	2·6	160

In some of these determinations I felt considerable uncertainty at what depth equality of colour was obtained; and some of the results were likely to vary on repetition. In the table I put 5·3 as the depth, after some hesitation and with the intention of repeating the experiment on some future occasion. On repetition, I made the depth of the disk 5·8, which would indicate the amount of salt used as 71. In some other instances I felt uncertainty when working with a solution of the bichromate in determining equality of tint; so that, whether the disk were elevated above or depressed below the position finally chosen, the difference in colour was insensible. Perhaps with bright yellow colours the eye requires more experience to detect differences of shade than with colours not so glaring. Moreover, when there is a considerable difference between the quantities of the colouring-salt in the two cylinders,

there is another difficulty to deal with: the colours to be compared differ not only in intensity but also in kind. A very dilute solution of bichromate gives a pale yellow with a slightly greenish shade, which passes, as the amount of salt is increased, into full yellow, orange-yellow, and finally orange. A similar observation holds with regard to several other colouring-salts. Thus, a very dilute solution of permanganate of potash is pink, passing, as the strength of the solution is increased, into crimson, and finally into violet. I also made some experiments with a blue solution. For this purpose I used an ammoniacal solution of sulphate of copper. The tinctorial power of this salt is not so great as I expected, and I made no experiments with a standard solution containing less than 150. On diluting 15 cub. c. of an ammoniacal solution with sufficient water to make 500 cub. c., I noticed after a little time that the solution was somewhat turbid; this was owing to the copper forming an insoluble compound, either hydrate or a subsalt; further addition of ammonia was necessary to clarify the solution. Standard solution 150 in 500 cub. c. of water (depth 8·3):—

A	B	C
200	5·7	218
220	5·3	235
240	5·1	244
260	4·8	259
280	4·5	277
300	4·1	304
350	3·4	366
400	3·0	415
450	2·6	479

Some experiments were made with solutions of known strength by sinking the disk to the depth assigned by theory, to see if the given tint was attained, instead of moving the disk about until the tint was estimated to be

the same, and then reading off the result. A solution of bichromate of potash, containing 15 in 500 cub. c., and depth 8·3, was used as a standard. Solutions containing 60, 70, 80, 90, gave similar tints at the calculated depths. Solutions containing 150 and increasing quantities, gave tints a little too deep at the calculated depths. Also a standard solution of permanganate of potash, containing 4·8 in 500 cub. c. of water and depth 8·3, was taken. Solutions containing 6 and 7·2 gave a similar tint at the depths assigned by the calculation. Solutions containing 9, 9·6, and higher quantities gave tints a little too deep at the calculated depths.

Some of the foregoing results are fair approximations; and probably better results might be obtained with a more perfect form of apparatus than I employed. The surfaces used were not perfectly flat. Moreover there was in some of the experiments the indecision arising from the coexistence of several distinct tints on the surface of the disk, due to the formation of caustic curves. This I afterwards remedied by working behind a screen of lawn. The effect of this is to render the illumination of the disk much more uniform. Instead of a screen we might also use glass cylinders ground internally. Another difficulty is the indecision respecting the depth at which equality of colour takes place. The degree of indecision will be different for different persons, and, I suppose, represents want of sensibility and want of skill in detecting shades of colour. So far as it arises from want of skill, it is likely to be diminished by more perfect experience with colours. It seems to me that it would be well to have some definite method for exercising the eye or for testing one's skill in this respect. This might be done by having a series of cylinders filled with coloured solutions, the quantity of colouring-matter increasing in arithmetical progression,

and endeavouring to arrange these solutions in their order, the arrangement being made according to the judgment of the tints. The various successes attending such exercises would afford the experimenter some method for testing his skill from time to time; it would also show him under what conditions he could use neither this nor any other method of colorimetry; for when he has arrived at such a stage that he can perceive no difference of tint, notwithstanding an increase in the colouring-matter, it seems as useless to try to do so as to use a balance of coarse sensibility to distinguish between two weights which are very nearly the same. By frequent exercise and concentrated attention, I think, the power of perception of difference of shade may be increased. If such a series of cylinders were taken, containing, say, quantities of bichromate of potash, increasing from 1 to 30, the first members of the series would be easily arranged, towards the middle of the series the number of successful discriminations would diminish, and few would be able to detect differences in the higher members of the series; for an increment of the salt which produces a considerable difference in intensity when a small quantity of the salt is present produces a small difference when the quantity is large. This would follow from the approximate formula used; for $\frac{dt}{dQ} = -\frac{c}{Q^2}$ (t = depth, Q = quantity of colouring-matter, c = constant); so that, for a given small difference in the quantity, the difference in the depth would be nearly proportional to the inverse square of the quantity. Solutions whose quantities of colouring-matter are so similar as to be beyond discrimination by colorimetry may be again brought within its range by the following device: in each of the the two cylinders destroy by some appropriate method an equal quantity of the colouring-matter; then their differences

will be increased relatively to the quantities of any colouring-matter remaining, and may be sufficiently sensible to be estimated.

When I suggested this method of colorimetry, I had not seen the article on Light by Sir John Herschel in the 'Encyclopædia Metropolitana.' He there gives a probable formula for the intensity of light which has passed through an absorbing medium. His reasoning is as follows:—Let the intensity of the incident light be unity, and its intensity on emerging from a unit thickness of the medium k; then its intensity at incidence on a second layer will be k, and its intensity at emergence k^2; hence, if t be the thickness of the medium, the intensity will be finally k^t, and if the original intensity were a, the final intensity would be ak^t. Now, light consists of several species; hence, if the composition of the incident light be $a_{\mathrm{I}}+a_{\mathrm{II}}+a_{\mathrm{III}}+\ldots$, &c., the composition of the emergent light will be $a_{\mathrm{I}}k_{\mathrm{I}}^t+a_{\mathrm{II}}k_{\mathrm{II}}^t+a_{\mathrm{III}}k_{\mathrm{III}}^t+\ldots$, &c. If we use the letter I to denote incident light, T to denote transmitted light, and A absorbed light, Herschel's formulæ may be written more briefly, as follows:—

$$\Sigma a=\mathrm{I},\quad \Sigma ak^t=\mathrm{T},$$

k will be a function of the quantity of colouring-matter present in a unit thickness. If g be that quantity, we may write $k=f(g)$, or, if we expand, $k=f(0)-f'(0)g+$higher terms in g. Now g is very small. Let, then, the terms higher than the second be neglected. The first term will be the light transmitted when no colouring-matter is present. If the colouring-matter were dissolved in an absolutely transparent medium, the first term would be unity. In practice water or some colourless medium is used; but the amount of light absorbed by a thin layer of pure water is very small; so that we shall make only a very small error

in writing $f(0)=1$. Hence $k=1-f'(0)g$. Substituting this value in Herschel's formula and expanding, we have

$$\Sigma a-\Sigma af'(0)gt+\ldots, \&c.$$

Now, as g is very small, we may neglect terms beyond the first two (unless t be very large). Hence

$$\Sigma af'(0)gt=I-T=A.$$

If we assume the colour to be constant, A will be a constant term. Now, let Q denote the whole quantity of colouring-matter in the cylinder, and L the whole length of the column; then $g=\frac{Q}{L}$, and the formula may be written

$$\Sigma af'(0)\frac{Qt}{L}=A, \text{ or } Qt=\frac{AL}{\Sigma af'(0)}=\text{constant}.$$

XXXIV. *The Distribution of Ammonia.*

By R. Angus Smith, F.R.S. &c.

Read April 30th, 1878.

If organic matter is everywhere, the presence of ammonia is everywhere possible; and if that matter is decomposing, ammonia is everywhere. That is the general statement which this paper illustrates. It is now many years since it was observed by me that organic matter could be found on surfaces exposed to exhalations from human beings; but it is not till now that the full significance of the fact has shone on me, and the practical results that may be drawn

from it in hygiene and meteorology. These results are the great extension of the idea that ammonia may be an index of decayed matter. The idea itself has been used partly, and to a large extent—as illustrated in my 'Air and Rain;' the facts now to be given enable us to claim for it a still more important place. The application seems to fit well the conditions already examined; and by this means currents from foul places have been readily found. This does not apply to the substances which may be called germs whether it be possible to see them or not, because these are not bodies which have passed into the ammoniacal stage, although some of them may be passing—those, for example, which are purely chemical and exert what we may call *idiolytic* action. This word—from ἴδιος, "its own (peculiar)," and λύσις, "decomposition"—may serve to mark this peculiar action, which was left by Liebig unnamed; he used the vague term invented by Berzelius, namely "catalytic." I have elsewhere recognized the two classes of germs, instead of any disputed one, without naming them.

It is now many years since Liebig first surprised me by saying that iron-ores and aluminous earths were capable of taking up ammonia, and if they were breathed upon we were able even to smell that substance. He, much about the same time, made numerous experiments in order to find the ammonia of the atmosphere, and to measure its amount in rain. The result for science was great; and Professor Way continued the inquiry for the Royal Agricultural Society. Dr. Gilbert, F.R.S., amongst his many labours in the department of agricultural science, has made this inquiry into ammonia of rain in still later times; but I shall not at present quote his results, as this paper is not intended to go fully into the subject, but rather to look in a different direction. The first paper I ever read

to this Society was on the ammonia found in peat; I was unable then to see the extent of the subject.

Ammonia must ever be one of the most interesting of chemical compounds. It comes from all living organisms, and is equally necessary to build them up. To do this, it must be wherever plants or animals grow or decay. As it is volatile, some of it is launched into the air on its escape from combination; and in the air it is always found. As it is soluble in water, it is found wherever we find water on the surface of the earth or in the air, and probably in all natural waters, even the deepest and most purified. As a part of the atmosphere, it touches all substances and can be found on many; it is in reality universally on the surface of the earth in the presence of men and animals, perhaps attached more or less to all objects, but especially to all found within human habitations, and, we might also add with equal certainty, the habitations of all animals.

If you pick up a stone in a city and wash off the matter on the surface, you will find the water to contain ammonia. If you wash a chair or a table or any thing in a room, you will find ammonia in the washing; and if you wash your hands you will find the same; and your paper, your pen, your table-cloth, and clothes, all show ammonia; and even the glass cover to an ornament has retained some on its surface. You will find it not to be a permanent part of the glass, because you require only to wash with pure water once or twice, and then you will obtain a washing which contains no ammonia; it is only superficial.

This ammonia on the surface is partly the result of the decomposition, continually taking place, of organic matter adhering to every thing in dwellings. The presence of organic matter is easily accounted for; but it is less easily detected than ammonia. It is probable that the chief cause of the presence of ammonia on surfaces in houses

and near habitations is the direct decomposition of organic matter on the spot. If so, being more readily observed than organic matter itself, it may be taken as a test, and the amount will be a measure of the impurity. A room that has a smell indicating recent residence will, in a certain time, have its objects covered with organic matter; and this will be indicated by ammonia on the surface of objects. After some preliminary trials, seeing this remarkable constancy of comparative results and the beautiful gradations of amount, it occurred to me that the same substance must be found on all objects around us, whether in a town or not. I therefore went a mile from the outskirts of Manchester and examined the objects on the way. Stones that not twenty hours before had been washed by rain, showed ammonia. It is true that the rain of Manchester contains it also; but, considering that only a thin layer would be evaporated from these stones, it was remarkable that they indicated the existence of any. The surface of wood was examined; palings, railings, branches of trees, grass (not very green at the time), all showed ammonia in no very small quantities. It seemed as if the whole visible surface around had ammonia. I went into the houses and examined the surfaces in rooms empty and inhabited, tables, chairs, walls, plates, glasses, and drawing-room ornaments. A (Parian) porcelain statuette under a glass showed some ammonia; a candlestick of the same material (but uncovered) showed much more. The back of a chair showed ammonia; when rubbed with a common duster, there was very little. It seemed clear that ammonia stuck to every thing.

If, then, ammonia was everywhere, the conclusion seemed to be that it was not at all necessary to do as I had been doing—namely, wash the air so laboriously; it would be quite sufficient to suspend a piece of glass and allow

the ammonia to settle upon it. For this purpose small flasks were hung in various parts of the laboratory, and examined daily. The flasks would hold about 6 ounces of liquid; but they were empty; and the outer surface was washed with pure water by means of a spray-bottle: it was done rapidly, and not above 20 cub. centims. (two thirds of an ounce) of water was used. This was tested for ammonia at once with the Nessler solution. The second washing, taken immediately, produced no appearance of ammonia. Ammonia could be observed after an hour and a half's exposure at any rate; but I do not know the shortest period. The results of the washings were as follows (they are the average of 34 experiments for some, and 17 for others—in all, 238 experiments):—

	Height from floor. ft. in.	Ammonia. mgm.	Height from floor. ft. in.	Ammonia. mgm.
Front laboratory	7 3	·013	4 2	·019
Second landing	6 0	·032		
Balance-room	5 1	·015	0 8	·009
First landing	4 10	·007		
Back laboratory	4 5	·010	0 6	·010
Entrance-lobby	6 5	·007		
Office.....	4 7	003		
Back yard.....	4 8	·036	0 7	·042
Back closet	2 3	·105		
Midden.....	.....	·572		

The first three, belonging to the working-laboratory, are not very regular, as we might suppose; but they never rise very high, nor do they sink to the lowest. The rest keep a remarkable similarity to each other from day to day; but among themselves the differences are very great. In the second there is a disturbance caused by sweeping the floors. On the other days it was requested that every thing should be kept still. This of course brings in a practical difficulty and limits the use of the test to cases where care can be used and thoughtful observation, since there are many ways

by which dust may be made to interfere, even although the act of sweeping should not take place. Still the dust-gradations themselves are worth measuring.

The result seems to be that a piece of glass of a definite size hung up in any place will receive deposits of ammonia or substances containing ammonia in a short time; and by washing the ammonia off with pure water and testing it with a Nessler solution, it may be seen whether there is too much or not. It is the simplest test for ammonia yet found. Its discoverer deserves great thanks. It must not be forgotten that we may have ammonia in very different conditions: it may be pure; or it may be connected with organic matter. This mode of inquiry is better suited as a negative test, to show that ammonia is absent, than to show what is present. When ammonia is present, there may be decomposing matter; when absent, there is not. I am hoping to make this a ready popular test for air—a test for sewer-gases*, for overcrowding, for cleanliness of habitations and even of furniture, as well as for smoke and all the sources of ammonia. Of course it must be used with consideration, and the conclusions must not be drawn by an ignorant person.

How far it may be used as a test of climate is a matter to be considered also.

After the above experiments I made another series of trials with air, Nesslerizing the washings at once, and not after laborious distillings, as in former cases; the results are very valuable, showing that we obtain comparative quantities equally in this way.

The amount of ammonia obtained in this ready way does not give exactly the same results as the more laborious methods which I have used; but it may be taken as the most convenient. It must be observed that the amount

* For sewage also to a larger extent than it has yet been used.

rises exactly where you might expect more organic matter to exist. The lowest is from Prince's Road, outside the town and almost half a mile from the extreme of the Manchester houses. The next is obtained from an empty yard behind my laboratory; but it is still pure, because there was wind and rain; and any one who observes how unusually pleasant it is to breathe the air even of a smoky town during rain and wind will not be surprised. I have not yet, however, had the purest air. I shall require to make a campaign on the moors, hills, and seas before I can give numbers for it. I have not even obtained the best on land at a distance from manufactures. All this will be done in time*.

In my office the amount is larger than outside. I shall give here a specimen of numbers obtained simply by washing a volume of air and Nesslerizing direct. These numbers do not compare with those already given. The highest amount was obtained at the front of the Cathedral, about midday on the 8th of February 1878, when the amount was 1·25, or 14½ times as much as had been found in Prince's Road, showing a considerable range.

Milligrammes of Ammonia per cubic metre of Air.

Prince's Road	·086
Open yard during rain	·119 and ·102
Front of laboratory	·167 ordinary.
Office	·167
Front and back during fog	·476
Close-shut-up room	·413
Closet outside	·800 to ·900
Densest part of fog	1·25

A fuller list may now be added to the above, which has already been published as an abstract.

* Since the paper was read I examined trees and stones at Skelmorlie and Wemyss Bay, finding very little ammonia.

Milligrammes of Ammonia (NH_3) *condensed upon Outside of Flasks of approximately equal Area suspended at undermentioned Places.*

[Flasks washed while suspended, and solution Nesslerized direct, without distillation; flasks left hanging without being dried.]

1878. February	8th.	9th.	11th.	12th.	13th.	15th.	18th.	19th.
			clear, frost.	clear.	clear.			
Front laboratory ..... 7 ft. 3 in. from floor	.009	·014	·0085	·005	·011	·026	·009	·029
" ..... 4 1 "	·007	·020	·008	·012	·014	·050	·027	·031
Second landing ..... 6 0 "	·008	·300	·010	·008	·012	·030	·015	·015
Balance-room ..... 5 1 "	.....	·136	·003	·004	·002	·006	·004	·005
" ..... 0 8 "	·008	·010	·004	·0035	·004	·008	·005	·0085
First landing ..... 4 10 "	·003	·023	·004	·0075	·004	·007	·007	·003
Back room ..... 4 5 "	·005	·065	·013	·005	·003	·008	·009	·005
" ..... 0 6 "	·004	·070	·003	·003	·003	·005	·005	·025
Entrance-lobby ..... 6 5 "	·007	·011	·008	·006	·004	·006	·006	·005
Laboratory office ..... 4 7 "	·004	·003	·003	·002	·002	·003	·005	·004
Back yard ..... 4 8 "	·056	·070	·058	·053	·025	·040	·035	·025
" ..... 0 7 "	·066	·078	·065	·060	·031	·038	·040	·030
Privy ..... 2 3 "	·225	·300	·270	·190	·100	·104	·058	·055
Over midden	·720	1·300	.....	·740	·300	1·100	·600	·300

I may add also an inquiry which I made previous to using the method just given. It will show the absorption of ammonia by water in various situations. It shows also that it is ammonia more than the decomposed matter which collects and thus gives an index to the amount of matter decomposed. Of course the ammonia comes, as a rule, from the decomposing matter, except in places where coal is burnt, when there is that additional source.

Absorption of Ammoniacal Substances from Air near the ground and other places by pure water placed in a basin under a bell jar.

[100 cubic centimetres of pure water, with a superficial area of 10·27 inches, were used for each experiment. These experiments were made during the year 1875.]

Place.	Weather.	Number of days the water was exposed.	Mgms. NH_3 in the exposed water.		Remarks.
			"Free."	"Albuminoid."	
On ashes in ash-pit	Wet every day.	5	1·499	0·024	
		7	0·450	0·010	Alkaline.
		7	0·380	0·018	Alkaline.
Mean	.....	.....	0·776	0·017	
Over ash-pit on a board 1 ft. above ashes.	.....	5	0·079	0·014	
Over ash-pit on a board 6 ft. above ashes.	Wet every day.	5	0·297	0·038	
		5	0·398	0·018	
		5	0·348	0·014	
		5	0·264	0·034	
		5	0·299	0·024	
		5	0·184	0·042	
		6	0·347	0·028	
Mean	.....	.....	0·305	0·028	

Absorption, &c. (continued).

Place.	Weather.	Number of days the water was exposed.	Mgms. NH_3 in the exposed water.		Remarks.
			"Free."	"Albuminoid."	
Wall by side of, and 10 feet above ashes in ash-pit.	.....	.....	0·399 0·197 0·303 0·138	0·029 0·019 0·180 0·009	Bell jar not fitting close to the wall upon which the water-vessel stood.
Mean	.....	.....	0·259	0·047	
Flag floor of privy.	Wet every day.	5 5	0·248 0·199 0·291	0·048 0·019 0·022	
Mean	.....		0·246	0·030	
On mud oozed from cistern.	Wet every day	5 5	0·849 0·589	0·062 0·242	Cistern receives part of house-drainage and rain.
Mean	.....	.....	0·719	0·152	
Cellar flags near to drain.	Raining during part of each day.	5 5 5 7	0·247 0·048 0·209 0·080	0·026 0·006 0·039 0·005	
Mean	.....	.....	0·146	0·019	
Inner-yard flags near to drain.	Raining on two days. Wet.	4 4 4 7	0·094 0·619 0·032 0·100	0·009 0·084 0·006 0·010	
Mean	.....	.....	0·211	0·027	
A hole in sandy soil 1 ft. deep in outer yard.	.....	5 5	0·199 0·067	0·024 0·029	Drainage runs onto this soil.
Mean	.....	.....	0·133	0·026	
Sandy soil from outer yard, 6 in. depth in a box.	Raining on two days.	4 4 4	0·021 0·007 0·015	0·011 0·009 0·008	Drainage runs onto this outer yard.
Mean	.....		0·014	0·009	

Absorption, &c. (continued).

Place.	Weather.	Number of days the water was exposed.	Mgms. NH_3 in the exposed water.		Remarks.
			"Free."	"Albuminoid."	
Kitchen flags, no fire and no work carried on in it.	Wet during part of each day.	5	0·279	0·019	
		5	0·058	0·004	
		7	0·120	0·007	
Mean	.....	.....	0·152	0·010	
Wooden floor in a 1st-floor room, but little used.	Rain on two days.	4	0·019	0·005	
		.....	?	0·014	
		5	0·029	0·009	
Mean	.....	.....	0·024	0·009	
Carpet on a wooden "ground-floor" office of laboratory.	.....	5	0·199	0·009	Room not much used during experiments.
		5	0·027	0·008	
Mean	.....	.....	0·113	0·0085	
Bedroom, on a table.	Dry.	7	0·080	0·005	
		7	0·100	0·005	
Mean	.....	.....	0·090	0·005	
On roof of house back of Devonshire Street, Manchester.	Dry.	7	0·100	0·008	Slightly alkaline.
		7	0·080	0·003	
	Wet.	7	0·040	0·003	
Mean	.....	.....	0·073	0·005	

The following are the mean results of the absorption of ammoniacal and nitrogenous bodies by water from air near the ground and other places:—

Place.	Weather.	Number of days the water was exposed.	Mgms. NH_3 in the exposed water.		Remarks.
			"Free."	"Albuminoid."	
On ashes in ash-pit (3 exp.).	Wet.	7, 7, 5	0·776	0·017	
On mud oozed from cistern (2 experiments).	Wet.	5	0·719	0·152	Cistern receives part of house-drainage and rain.
Over ash-pit, on a board 6 ft. above ashes (7 exp.).	Wet.	5	0·305	0·028	
Wall by side of ash-pit, 10 ft. above ashes (4 exp.).	.....	5	0·259	0·047	
Flag floor of privy (3 experiments).	Wet.	5	0·246	0·030	
Inner yard flags, near drain (4 experiments).	Wet part of time.	4, 4, 4, 7	0·211	0·027	
Cellar flags, near drain (4 experiments).	Wet part of time.	5, 5, 5, 7	0·146	0·019	
A hole in sandy soil 1 ft. deep in outer yard (2 experiments).	.....	5	0·133	0·026	Drainage runs onto this soil.
Kitchen-flags, no fire and no work carried on in it (3 experiments).	Wet.	5, 5, 7	0·152	0·010	
Carpet on a wooden "ground-floor" office of laboratory.	.....	5	0·113	0·008	Room very little used.
Bedroom, on a table (2 exp.).	Dry.	7	0·090	0·005	
On roof of house at back of All Saints' (3 exp.).	Dry.	7	0·073	0·005	
Wooden floor in a "1st-floor" room.	.....	.....	00·24	0·009	Room very little used.
Sandy soil from outer yard, 6 in. depth of it in a box (3 exp.).	.....	.....	0·014	0·009	

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1867, Feb. 5. Cialdi, Alessandro, Commander, &c. *Rome.*

1870, Mar. 8. Cockle, The Hon. Sir James, M.A., F.R.S., F.R.A.S., F.C.P.S., *Sandringham-gardens, Ealing.*

1866, Jan. 23. De Caligny, Anatole, Marquis, Corresp. Mem. Acadd. Sc. Turin and Caen, Socc. Agr. Lyons, Sci. Cherbourg, Liége, &c.

1861, Apr. 2. Durand-Fardel, Max, M.D., Chev. of the Legion of Honour, &c. 36 *Rue de Lille, Paris.*

1849, Apr. 17. Girardin, J., Off. Legion of Honour, Corr. Mem. Imper. Instit. France, &c. *Lille.*

1862, Jan. 7. Gistel, Johannes Franz Xavier, Ph.D., late Prof. of Nat. Hist. and Geogr., Libr. Secr. and Conserv. at the Museum of Nat. Hist. Regensburg, &c. 19 *Steinweg, Regensburg, Bavaria.*

1850, Apr. 30. Harley, Rev. Robert, F.R.S., F.R.A.S. *Burton Bank, Mill Hill, Middlesex, N.W.*

1816, Apr. 26. Kenrick, Rev. John, M.A. *York.*

1838, Apr. 17. Koechlin-Schouch, Daniel. *Mulhouse.*

1862, Jan. 7. Lancia di Brolo, Federico, Duc. Inspector of Studies, &c. *Palermo.*

1859, Jan. 25. Le Jolis, Auguste-François, Ph.D., Archiviste perpétuel and late President of the Imper. Soc. Nat. Sc. Cherbourg, &c. *Cherbourg.*

1857, Jan. 27. Lowe, Edward Joseph, F.R.S., F.R.A.S., F.G.S., Mem. Brit. Met. Soc., &c. *Nottingham.*

1862, Jan. 7. Nasmyth, James, C.E., F.R.A.S., &c. *Penshurst, Tunbridge.*

1851, Apr. 29. Pincoffs, Peter, M.D., Knt. of the Turkish Order of the "*Medjidié*" 4th Cl., Mem. Coll. Phys. London, Brussels, and Dresden, &c. *Naples.*

1869, Jan. 12. Saint Venant, Barré de, Ingénieur en chef des Ponts et Chaussées, Corr. Soc. Sci. de Lille et de l'Acad. Romaine des *Nuovi Lincei*, &c.

1867, Feb. 5. Schönfeld, Edward, Ph.D., Director of the Mannheim Observatory.

1834, Jan. 24. Watson, Henry Hough. *Bolton, Lancashire.*

ORDINARY MEMBERS.

DATE OF ELECTION.

1861, Jan. 22. Alcock, Thomas, M.D., Extr. L.R.C.P. Lond., M.R.C.S. Engl., L.S.A. *Oakfield, Ashton-on-Mersey.*

1873, Jan. 7. Allmann, Julius. 2 *Victoria-street.*

1870, Dec. 13. Angell, John. *Manchester Grammar School.*

1861, Jan. 22. Anson, Ven. Archd. George Henry Greville, M.A. *Birch Rectory, Rusholme.*

1837, Aug. 11. Ashton, Thomas. 36 *Charlotte-street.*

1874, Nov. 3. Axon, William E. A., M.R.S.L., F.S.I.

1865, Nov. 15. Bailey, Charles. 85 *Withington-road, Whalley Range.*

1824, Jan. 23. Barbour, Robert. 18 *Aytoun-street.*

1876, Nov. 28. Barratt, Walter Edward. *Kersal, Higher Broughton.*

1867, Nov. 12. Barrow, John. 3 *Egerton-terrace, Birch-lane, Longsight.*

1858, Jan. 26. Baxendell, Joseph, F.R.A.S., Corr. Mem. Roy. Phys. Econ. Soc. Königsberg, and Acad. Sc. & Lit. Palermo. 14 *Liverpool-road, Birkdale, Southport.*

1847, Jan. 26. Bazley, Sir Thomas, Bart., M.P. *Stow-on-the-Wold, Gloucestershire.*

1877, Nov. 27. Becker, Wilfred, B.A. 4 *Commercial Buildings, Cross-street.*

1878, Nov. 26. Bedson, Peter Phillips, D.Sc. *Owens College.*

1847, Jan. 26. Bell, William. 51 *King-street.*

1870, Nov. 15. Bennion, John A., F.R.A.S. *Corpus Christi College, Cambridge.*

1858, Jan. 26. Benson, Davis. 4 *Chester-street.*

1868, Dec. 15. Bickham, Spencer H., jun. *Endsleigh, Alderley Edge.*

1842, Jan. 25. Binney, Edward William, F.R.S., F.G.S., Hon. Mem. Geol. Socs. Edinburgh and Liverpool, and Geol. and Polytech. Soc. West Riding of Yorkshire. 55 *Peter-street.*

1870, Nov. 15. Bird, John Durham, M.D. *Heaton Chapel.*

1876, April 18. Birley, Thomas Hornby. *Somerville, near Manchester.*

1821, Jan. 26. Blackwall, John, F.L.S. *Hendre, Llanrwst.*

1861, Jan. 22. Bottomley, James, D.Sc., B.A., F.C.S. 7 *Irwell-terrace, Lower Broughton.*

1839, Oct. 29. Bowman, Henry. *Brooklands, Brockham Green, Reigate, London.*

1875, Nov. 16. Boyd, John. 58 *Parsonage-road, Withington.*

DATE OF ELECTION.

1873, Nov. 4. Bridson, Joseph Ridgway, F.R.A.S. *Bolton.*

1855, April 17. Brockbank, William, F.G.S. *Prince's Chambers, 26 Pall Mall.*

1861, April 2. Brogden, Henry. *Hale Lodge, Altrincham.*

1844, Jan. 23. Brooks, William Cunliffe, M.A., M.P. *Bank, 92 King-street.*

1860, Jan. 24. Brothers, Alfred, F.R.A.S. 14 *St.-Ann's-square.*

1846, Jan. 27. Browne, Henry, M.D., M.A., M.R.C.S. Engl. 244 *Oxford-street.*

1872, Nov. 12. Burghardt, Charles Anthony, Ph.D. 110 *King-street.*

1874, Dec. 15. Carrick, Joseph. *Prince's Chambers, 26 Pall Mall.*

1858, Jan. 26. Casartelli, Joseph. 43 *Market-street.*

1852, April 20. Chadwick, David, F.S.S., Assoc. Inst. C.E., M.P. 2 *Moorgate-street, London, E.C.*

1842, Jan. 25. Charlewood, Henry. 1 *St. James's-square.*

1854, April 18. Christie, Richard Copley, M.A., Prof. Hist. Owens College. 2 *St.-James's-square.*

1841, April 30. Clay, Charles, M.D., Extr. L.R.C.P. London, L.R.C.S. Edin. 101 *Piccadilly.*

1853, Jan. 25. Cottam, Samuel, F.R.A.S. 6 *Essex-street.*

1859, Jan. 25. Coward, Edward. *Heaton Mersey, near Manchester.*

1861, Nov. 12. Coward, Thomas. *Bowdon.*

1861, April 29. Crompton, Samuel, M.R.C.S. Eng., L.S.A., F.R. Med. Chir. Soc. 24 *St.-Ann's-square.*

1848, Jan. 25. Crowther, Joseph Stretch. 20 *St.-John's-street.*

1876, April 18. Cunliffe, Robert Ellis. *Fern Hill, Chaseley-road, Pendleton.*

1861, April 2. Cunningham, William Alexander. *Wiston, Biggar, N.B.*

1854, Feb. 7. Dale, John, F.C.S. *Cornbrook Chemical Works, Chester-road.*

1871, Nov. 8. Dale, Richard Samuel, B.A. *Cornbrook Chemical Works, Chester-road.*

1842, April 19. Dancer, John Benjamin, F.R.A.S. *Manor House, Tipping-street, Ardwick.*

1853, April 19. Darbishire, Robert Dukinfield, B.A., F.G.S. 26 *George-street.*

1878, Nov. 26. Davies, Joseph. *Engineer's Office, Lancashire and Yorkshire Railway, Hunt's Bank.*

1869, Nov. 2. Dawkins, William Boyd, M.A., F.R.S. *Museum, Owen's College.*

DATE OF ELECTION.

1861, Dec. 10. Deane, William King. 25 *George-street.*
1879, Mar. 18. Dent, Hastings Charles. 112 *Bury New Road.*

1878, Mar. 5. Fairgrieve, Andrew. 21 *Brisbane-street, Greenock.*
1872, April 16. Freeston, Rev. Joseph. *Chapel-lane, Blackley.*

1873, Nov. 18. Gamgee, Arthur, M.D., F.R.S., Professor of Practical Physiology and Histology, Owens College. *Owens College.*
1877, Feb. 6. Garnett, William. *Quernmore Park, Lancaster.*
1840, Jan. 21. Gaskell, Rev. William, M.A. 46 *Plymouth-grove.*
1874, Nov. 3. Grimshaw, Harry, F.C.S. *Thornton-view, Clayton.*
1875, Feb. 9. Gwyther, R. F., B.A., Lecturer on Mathematics, Owens College. *Owens College.*

1878, Oct. 1. Hannay, James Ballantyne, F.R.S.E. *Woodburne, Helensburgh, Glasgow.*
1878, April 30. Harland, William Dugdale. 25 *Acomb-street, Greenheys.*
1871, Oct. 3. Harrison, Thomas. 18 *St.-Ann's-square.*
1862, Nov. 4. Hart, Peter. 192 *London-road.*
1839, Jan. 22. Hawkshaw, Sir John, F.R.S., F.G.S., Mem. Inst. C.E. 33 *Great George-street, Westminster, London, S.W.*
1873, Dec. 16. Heelis, James. 71 *Princess-street.*
1828, Oct. 31. Henry, William Charles, M.D., F.R.S. 11 *East-street, Lower Mosley-street.*
1868, Nov. 17. Herford, Rev. Brooke. 470 *Michigan Avenue, Chicago, U.S.*
1833, April 26. Heywood, James, F.R.S., F.G.S., F.S.A. 26 *Kensington-Palace Gardens, London, W.*
1864, Mar. 22. Heywood, Oliver. *Bank, St.-Ann's-street.*
1851, April 29. Higgin, James. *Little Peter-street, Gaythorn.*
1839, Jan. 22. Hobson, John. *Rockville, Ballyshannon, Donegal.*
1854, Jan. 24. Holcroft, George. *St.-Mary's-gate.*
1855, Jan. 23. Holden, Isaac. 64 *Cross-street.*
1846, Jan. 27. Holden, James Platt. 56 *Johnson-street, Smedley Lane, C. Hill.*
1873, Dec. 2. Howorth, Henry H., F.S.A. *Derby House, Eccles.*
1857, Jan. 27. Hunt, Edward, B.A., F.C.S. 42 *Quay-street, Salford.*
1855, Jan. 25. Hurst, Henry Alexander. *The Albany, Liverpool.*

1872, Feb. 6. Jewsbury, Sidney. 39 *Princess-street.*
1850, April 30. Johnson, Richard, F.C.S. *Oak Bank, Fallowfield.*

DATE OF ELECTION.

1870, Nov. 1. Johnson, William H., B.Sc. 26 *Lever-street.*
1878, Nov. 26. Jones, Francis. *Grammar School.*
1848, April 18. Joule, Benjamin St. John Baptist, J.P. 12 *Wardle-road, Sale.*
1842, Jan. 25. Joule, James Prescott, D.C.L., LL.D., F.R.S., F.C.S., Hon. Mem. C.P.S., &c. 12 *Wardle-road, Sale.*

1852, Jan. 27. Kennedy, John Lawson. 47 *Mosley-street.*
1862, April 29. Knowles, Andrew. *High Bank, Pendlebury.*

1860, Jan. 24. Latham, Arthur George. *Lower King-street.*
1863, Dec. 15. Leake, Robert. 75 *Princess-street.*
1850, April 30. Leese, Joseph. *Altrincham.*
1857, Jan. 27. Longridge, Robert Bentink. *Yew Tree House, Tabley, Knutsford.*
1870, April 19. Lowe, Charles. 61 *Piccadilly.*
1850, April 30. Lund, Edward, F.R.C.S. Eng., L.S.A. 22 *St.-John's-street.*
1859, Jan. 25. Lynde, James Gascoigne, M. Inst. C.E., F.G.S. *Town Hall.*

1866, Nov. 13. McDougall, Arthur. 68 *Port-street.*
1875, Oct. 5. Mackereth, Rev. Thomas, F.R.A.S., F.M.S. *Highbank, Belmont-road, Bolton.*
1859, Jan. 25. Maclure, John William, F.R.G.S. *Cross-street.*
1875, Jan. 26. Mann, John Dixon, Licentiate of the King and Queen's College of Physicians, Ireland; and M.R.C.S. Eng. 16 *St.-John's-street.*
1873, Nov. 4. Marshall, Rev. William, B.A. 6 *Swinton Avenue, Plymouth-grove.*
1878, Nov. 26. Martin, Sidney Trice. 66 *Major-street.*
1864, Nov. 1. Mather, William. *Iron Works, Deal-street, Brown-street, Salford.*
1868, Nov. 17. Mawson, John Isaac, C.E. 4 *Essex-street.*
1837, Jan. 27. Mellor, William. 3 *Hyde-road, Ardwick.*
1873, Mar. 18. Melvill, James Cosmo, M.A., F.L.S. 6 *North Corridor, Royal Exchange.*
1877, Nov. 27. Moore, Samuel. 25 *Dover-street, C.-on-M.*
1861, Oct. 29. Morgan, John Edward, M.B., M.A., M.R.C.P., Lond., F.R. Med. and Chir. S. 1 *St. Peter's-square.*

1854. Feb. 7. Nevill, Thomas Henry. 19 *George-street.*
1850, Jan. 24. Newall, Henry. *Hare-hill, Littleborough.*
1873, Mar. 4. Nicholson, Francis, F.Z.S. 62 *Fountain-street.*

DATE OF ELECTION.

1875, Nov. 16. Nix, E. W., M.A. *Branch Bank of England, King-street.*

1862, Dec. 30. Ogden, Samuel. 10 *Back Mosley-street.*
1861, Jan. 22. O'Neill, Charles, F.C.S., Corr. Mem. Ind. Soc. Mulhouse. 72 *Denmark-road.*
1844, April 30. Ormerod, Henry Mere. 5 *Clarence-street.*

1861, April 30. Parlane, James. *Rusholme.*
1876, Nov. 28. Parry, Thomas. *Grafton-Place, Ashton-under-Lyne.*
1874, Jan. 13. Pennington, Rooke, LL.B., F.G.S. *Mawdsley-street, Bolton.*
1854, Jan. 24. Pochin, Henry Davis. *Barn Elms, Barnes, Surrey, S.W.*
1860, April 17. Pocklington, Rev. Joseph Nelsey, B.A. *Rectory, St. Michael's, Hulme.*
1877, Feb. 6. Poynting, John Henry, B.A., B.Sc. *Monton, Eccles.*
1878, Oct. 1. Priestley, John. *Owens College.*

1861, Jan. 22. Radford, William. 41 *John-Dalton-street.*
1854, Feb. 7. Ramsbottom, John. *Fern Hill, Alderley Edge.*
1859, April 19. Ransome, Arthur, M.A., M.D. Cantab., M.R.C.S. 1 *St.-Peter's-square.*
1869, Nov. 16. Reynolds, Osborne, M.A., F.R.S., Professor of Engineering, Owens College. *Owens College.*
1860, Jan. 24. Roberts, William, M.D., B.A., F.R.S., M.R.C.P. Lond. 89 *Mosley-street.*
1864, Dec. 27. Robinson, John. *Atlas Works, Great Bridgewater-street.*
1822, Jan. 25. Robinson, Samuel. *Blackbrook Cottage, Wilmslow.*
1858, Jan. 26. Roscoe, Henry Enfield, B.A., LL.D. Ph.D., F.R.S., F.C.S., Professor of Chemistry, Owens College. *Owens College.*

1851, April 29. Sandeman, Archibald, M.A. *Tullock, near Perth.*
1870, Dec. 13. Schorlemmer, Carl, F.R.S., F.C.S. *Owens College.*
1842, Jan. 25. Schunck, Edward, Ph.D., F.R.S., F.C.S. *Oaklands, Kersal.*
1873, Nov. 18. Schuster, Arthur, Ph.D., F.R.S. *Sunnyside, South Hampstead, London.*
1852, April 20. Sidebotham, Joseph, F.R.A.S. *The Beeches, Bowdon.*
1838, Jan. 26. Smith, George Samuel Fereday, M.A., F.G.S. *Grove House, Tunbridge Wells.*

DATE OF ELECTION.

1876, Nov. 28. Smith, James. 19 *Cleveland-road, Crumpsall.*

1844, April 29. Smith, Robert Angus, Ph.D., F.R.S., F.C.S., Corr. Mem. I.R. Geol. Inst. Vienna. 22 *Devonshire-street, All Saints.*

1859, Jan. 25. Sowler, Thomas. 24 *Cannon-street.*

1851, April 29. Spence, Peter, F.C.S., M.S.A. *Alum Works, Newton Heath.*

1864, Dec. 27. Spencer, Joseph. 105 *Portland-street.*

1870, Nov. 1. Stewart, Balfour, LL.D., F.R.S., Professor of Natural Philosophy, Owens College. *Owens College.*

1863, Oct. 6. Stretton, Bartholomew. *Bridgewater-place, High-street.*

1870, Mar. 22. Teale, James. *Springfield, Sale.*

1873, April 15. Thomson, William, F.R.S.E. *Royal Institution.*

1860, April 17. Trapp, Samuel Clement. 88 *Mosley-street.*

1873, Nov. 18. Waters, Arthur William, F.G.S. *Woodbrook, Alderley Edge.*

1874, Dec. 15. Watson, Morrison, M.D., Professor of Descriptive Anatomy, Owens College. *Linda Villa, Heald-road, Bowdon.*

1874, Jan. 27. Watts, John, Ph.D. 23 *Strutt-street.*

1857, Jan. 27. Webb, Thomas George. *Glass Works, Kirby-street, Ancoats.*

1858, Jan. 26. Whitehead, James, M.D., M.R.C.P. Lond., F.R.C.S. Engl., L.S.A., M.R.I.A., &c. 87 *Mosley-street.*

1839, Jan. 22. Whitworth, Sir Joseph, Bart., F.R.S. *Chorlton-street, Portland-street.*

1859, Jan. 25. Wilde, Henry. *Mill-street, Ancoats.*

1859, April 19. Wilkinson, Thomas Read. *Manchester and Salford Bank, Mosley-street.*

1874, Nov. 3. Williams, William Carleton, F.C.S. *Owens College.*

1851, April 29. Williamson, William Crawford, F.R.S., Professor of Nat. Hist., Anat., and Physiol., Owens College, M.R.C.S. Engl., L.S.A. *Egerton-road, Fallowfield.*

1871, Oct. 31. Winstanley, David, F.R.A.S. *Doctor's Cottage, South Shore, Blackpool.*

1836, Jan. 22. Wood, William Rayner. *Singleton Lodge, near Manchester.*

1860, April 17. Woolley, George Stephen. 69 *Market-street.*

1863, Nov. 17. Worthington, Samuel Barton, C.E. 12 *York-place, Oxford-street.*

DATE OF ELECTION.

1865, Feb. 21. Worthington, Thomas. *Bridgewater-Club Chambers, King-street.*

1864, Nov. 1. Wright, William Cort, F.C.S. *Alexandra-place Heaton Norris.*

N.B.—Of the above list the following have compounded for their subscriptions, and are therefore Life Members:—

Brogden, Henry.
Johnson, William H., B.Sc.
Sandeman, Archibald, M.A.
Smith, Robert Angus, Ph.D., F.R.S.

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DLE MAY NOT BE

WIDENER
AUG 21 2002
CANCELLED